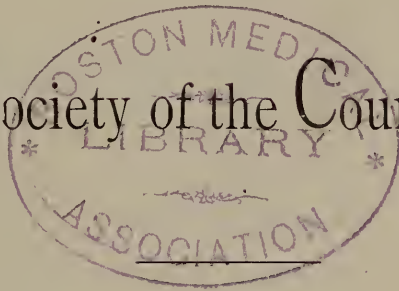


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No. I.

FORCED ALIMENTATION.*

BY ALEXANDER HUTCHINS, M.D.

Systematic feeding in the course of exhausting diseases, in convalescence, and in the treatment of dyspepsia, is no novelty. The literature is ample, and the practice followed by all judicious and skillful practitioners. The theory in the former conditions is, that the rapid waste must be promptly made good, and, in the last, that the weakened or perverted digestive functions must be judiciously supplied with appropriate food.

It is the popular opinion that food should not be taken when the desire for food does not exist. It is the object of this paper to hint at certain clinical facts that prove the contrary of this opinion, and point to forced alimentation as a therapeutic agent. It is possible that the practice herein indicated may be generally followed; but, if so, even the fugitive literature of the profession is barren of the facts.

That impaired nutrition is associated with a wide range of chronic diseases, and with that well-nigh undefinable condition of "poor health," and that the sufferers from either are generally irregular, fickle and imperfect feeders, are propositions that need but to be stated. It may be that the disease has broken down the appetite for food, and thus the irregularity; or it may be the irregularity, long continued, has rendered

* Read before the Medical Society of the State of New York, February 3rd, 1880.

the disease possible, or directly accounts for the general discomfort. In either case the nutrition is at fault, and the point in practice is to recover the nutrition.

An *individual* with an intelligent and vigorous appetite, whose nutritive functions are accurately performed, one meets with, but not too often; a *family* that gathers round a judiciously furnished table, at regular intervals, and does uniform justice to the spread, with zest and energy, one meets with but rarely. *Hinc illæ lachrymæ.*

It is no purpose of these moments to portray the consequences of ill-feeding; but to suggest that frequently a well directed feeding will be the means of restoration to physical vigor and freedom from disease.

Starvation may happen suddenly from a total deprivation from food. There are many who are passing their lives in a state of semi-starvation from an insufficiency of food. There may be no desire for food, and yet it may be possible to cultivate the desire.

The amount of needful food varies with a large variety of conditions. In any special case it will not be difficult to discover whether the needful amount is taken and appropriated. The question in therapeutics is, how is it possible to cause the needful amount to be taken and appropriated? The opposition, therefore, may be physical or moral. The stomach may loathe or it may reject food. The necessity not being known to the individual, there may be no effort to take food. The necessity being known, the will power may be too weak to enforce it.

Gluttony on the one hand and an inadequacy of food on the other are, to a large extent, habits. In either case a peculiar condition of the system is acquired. So far as the nutrition is concerned, the system accommodates itself to the insufficient supply. The resultant condition is not the best, for divers ills result.

The stomach gets the blame; but while it is at fault all the other members suffer with it. The stomach and other organs that participate in the digestion and assimilation of food, are as much the objects of nutrition as the organs or parts of the economy most remote from those connected with the appropriation of food. There is an interdependence. The stomach receives food, and is supported by the food.

So the desire for food is prompted, not merely by the ability of the stomach to receive food, but by such continued and uniform general nutrition that the ordinary and normal waste sends back to the stomach the demand for food by which, ultimately, the waste is to be repaired.

Of course, where the desire for food does not exist, and little is taken, the waste is proportionately diminished. A certain condition of imperfect health is maintained. The conditions exist for the onset of functional disorder. Malaise and general discomfort are present.

The point is, that there is a large variety of disordered conditions, unaccompanied by organic disease, to a very large extent the result of defective nutrition, where the amount of food taken is small in quantity, improper as to character, irregular as to time, matters of long standing, which chronic habit of insufficient feeding can be overcome by a properly selected dietary, systematically arranged and rigidly enforced, with the result of a speedy restoration to health and vigor, which result cannot ordinarily be effected without this forced alimentation, and this without the intervention of drugs.

The clinical question is, can these conditions of disorder, which are accompanied by an imperfect nutrition dependent on an insufficiency of food, be overcome by forced feeding that shall compel the system to take and appropriate a sufficiency of food? Will the systematic and forcible occupation of the stomach by food finally beget the normal appetite for food?

How far is drug-taking efficiently related to forced feeding? Should it precede, accompany, or come in as an adjunct, after the habit of taking food has begun to be established?

These are questions that can only be met by the study of each individual case. Many cases can be conducted up to a sound digestion by the use of food alone. The habit can be established. The end aimed at is the proper digestion of an ample amount of food. Drugs are not, and cannot be made to be, a substitute for food; and drugs, in a large class of cases, are valueless for the establishment of an appetite.

If there is one position that the physician can occupy as the skilled and competent adviser, it certainly is in the exercise of his experienced authority in constructing the details of a dietary, and insisting on implicit and unquestioning obedience thereto. No general advice will avail. Patients, who will submit to any extremity of medication, and adhere to directions with pertinacity, will rebel against the details of forced feeding, shake their heads ominously at the prospective labor, or flatly refuse to undertake the purgatorial discipline. So, if any success is to be anticipated from the treatment, the physician must descend from the lofty tribune whence general principles are promulgated, and patiently, and with calm assurance, give his patient the most precise and intimate directions. Authority and persuasion must go hand in hand. In the multitude of homes, where the indiscretion of parents has relaxed authority in the matter of feeding, and a race of imperfectly nourished children is losing vigor day by day, to demand that these parents shall revolutionize themselves, insist on their children eating a proper breakfast, provide a suitable lunch for the school recess and make sure that it is eaten, and follow up the regime till the end is gained, is a revolution in morals

the magnitude of which is known only to the physician whose intelligence and conscience have determined him to carry it through. An ill-fed and imperfectly nourished childhood will arouse his anxiety and stimulate his zeal, and the result aimed at can be accomplished.

The recognition of this principle of forced feeding as a therapeutic measure is a valuable adjunct in resisting certain influences of heredity. The inheritors of certain constitutional taints are notoriously light and fickle feeders. The parents themselves, unconscious of the discipline of food, accept their offsprings' "weak stomach" as the inevitable. But this "weak stomach" is also an uneducated stomach, with latent possibilities that a cultivated discretion may call into vigorous exercise. This digestion, not stimulated by drugs, but exercised by materials that possess the elements of nutrition, can enforce a nutrition by which the malign hereditary influences may be set aside, and the maturing years be coincident with maturing powers of resistance that would be unknown to the period of development, ill-trained and ill-nourished. Bearing in mind that food is the natural tonic, and that drug tonics are but excitants to the natural tonics, it, at the same time, is not to be lost sight of that, during the entire period of growth and development, where waste and repair go on so rapidly, where accumulation stretches way up beyond the region of supply, and that for a long continuous succession of years, any protracted failure of appetite and an inadequacy of food, coincident with a want of enthusiasm and buoyancy native to vigorous youth, must naturally suggest the most gloomy forebodings. In the most unpromising cases much can be accomplished. The end sought can be obtained by a patient, uncompromising, intelligent attack on the habit of insufficient feeding. The physician must take on himself the self-denial and profound convictions of a missionary, for the most obdurate of heathen is an over-indulgent, undisciplined and blindly-affectionate parent. In view of parental responsibility, abject ignorance and culpable neglect were the better terms. She must first be converted, that his commands may be implicitly obeyed and relentlessly enforced.

I shall not undertake to describe the details of a procedure that must, of necessity, be modified by a variety of contingencies. The conditions are not as when food is administered for the purpose of repairing the rapid waste of exhausting disease, and the attention is not directed to such conditions as must be considered in satisfying the appetite of a dyspeptic by a judiciously regulated diet. To overcome the habit of not taking food (insufficient feeding), experience proves that it is less a question, at the outset, of what shall be eaten than that something shall be eaten.

Aside from all considerations of appetite, notwithstanding any weakness

of will-power, or the multiplied obstacles of repulsion, loathing or seeming impossibilities, the first point is to get food of some kind into the stomach, in such quantities as is possible and at stated intervals, as a stern duty, either in fulfillment of an intelligent appreciation of the necessity, or in submissive obedience to inflexible orders. The operation is at first strictly mechanical, and being such, the physician should disregard the sense of taste with the same indifference that he would exercise in the administration of medicine in an emergency. In point of fact, the neutral tint is the most satisfactory. In the selection, food that requires mastication should be avoided, and food, without special flavor, that can be swallowed by a single effort, should be selected. *Ce n'est que le premier pas que coule.* The first successful effort, and the first effort can always be successful, is a demonstration to the patient of ultimate success; is a conversion of sentiment; is a tonic to the will.

As a second consideration, solids and semi-solids have the preference over liquid foods. It is a familiar physiological fact that the contact of solids arouses very rapidly the special functions of the stomach, and, as a matter of clinical observation in forced feeding, the solids and semi-solids are better borne than liquids. Again, it must be borne in mind that the effort is not to assist a weak digestion, but to habituate the stomach to the presence of food, and therewith to provoke its activity. What follows will be always the result of judicious selection.

If there be one direction to be followed in forced feeding that is essential above all others, it is that this feeding should be conducted at regular intervals (the frequency to be regulated by the quantity taken), stretching over a long period of each day. As a matter of clinical observation it has been found that this feeding is best practiced 18 hours out of the 24. As a rule, also, this feeding should be commenced before the patient is out of bed in the morning—the earlier the better. In an ill-nourished person the change of position from supine to erect, and the effort of dressing, develop a condition allied to faintness; and from this cause, or some other, with these patients, there is, in the morning, the greatest disinclination to take food. It will surprise any who have not practiced this habit of taking some food in bed, so early, if possible, as to secure a nap before rising, to observe how quickly the disinclination for the breakfast meal will lessen. Insisting upon stated intervals, unvarying as the clock, is of paramount importance. This admits of no concession to a weakened will, and it encourages a physical habit.

Although so much is claimed for milk and lime-water as the panacea for all nutritive ills, yet this combination does not fulfill all the indications of forced feeding. The nutrition, as I am describing it, is suspended, and not degraded. It is less oppressed than inactive. The

stomach is not at once ready to endure or digest food. It is not functionally disordered, but torpid. The emulsion is useful, but not universally so.

The caution is, perhaps, needless, that the nitrogenized foods are inappropriate in the early stages of forced feeding. The general inactivity of the entire digestive apparatus renders it in a condition unfit to digest and appropriate them. In general, the coarser farinaceous foods fulfill the indications of bulk and an excess of refuse material stimulating the alimentary canal and its adjuncts throughout its entire length. The quantity, of course, at the outset must be small, to be increased as circumstances will permit.

The free use of water is to be insisted on as a requisite to success in this forced feeding. In ill-nourished persons there is well nigh universally a marked diminution of the secretions. A large admixture of water with the food is of great use in promoting absorption, in encouraging the action of the kidneys, in softening the dejecta, and in setting free the action of all the emunctories. What is useful in ordinary health becomes a necessity when so radical a measure is entered upon as is here described.

These forcible measures for subverting a dyscrasia and substituting therefor a normal condition of animal life, must be synchronous with submission to all those conditions which are justly recognized as influencing free animal action, regular evacuations, abundant fresh air, judicious exercise, cheerful occupation.

To bring forward an array of clinical experience in support of this therapeutics would be a wearisome detail. I have been persistent in this practice for many years, and each week has some new story to tell. A few lines would tell the tiresome monody, ever and again repeated. The child peevish and playless, shrunken, and older than beseems his days; the tired school-girl, with her headache, and lassitude, and unrefreshing sleep; the maiden whose years fit her for the sunshine, apprehensive, oppressed and shrinking into cloistered retirement; young maturity aimlessly struggling on under a burden of infinite malaise; the later years, unburdened by actual disease, but drifting along through weary periods of functional distress; a wan and weary host, under manifold variety of guise, each with a peculiar story begotten of a special idiosyncrasy, all stripped of their enthusiasm, buoyancy, vigor and comfort, with one common, sickening wail of vanished appetite for food.

It is possible that to a portion, at least, of this audience some contrition must needs be expressed for presenting a subject with so dreary a monotone, whose details comprise but the ordinary procedure of intelligent work. But it must be recalled that to the large rank and file of

medical practitioners there are anxieties, demands and expectations, whose worry and exactions have been forgotten, or never experienced, by some whose responsibilities and daily bread, by reason of their standing *sub magni nominis umbra*, are separated in the less disturbed region of defined and transient consultation, from the complications that beset the majority.

There is no class of invalids to whom mere general and occasional advice is more worthless, and, certainly, none who make more persistent demands upon, and are more harassing to, their medical adviser. If forced alimentation can, on the one hand, assist in the recovery from certain forms of chronic invalidism, and, on the other, avert the consequences and inroads of more distinctive disease, the suggestion of this therapeutic measure may, perchance, lengthen the list of curable disorders.

A CASE OF LABOR COMPLICATED BY A NARROW PELVIS, AND PROLAPSE OF THE CORD—CRANIOTOMY, WITH REMARKS.

BY J. J. LAMADRID, M.D.

I call your attention to a case, whose history, I trust, will be as interesting to you as it has been instructive to me. It is well-known that these cases are fortunately rare; indeed, a physician may practice a life time, and never attend a case, or be compelled to pass through the painful ordeal and anxiety which they naturally cause.

December 21st, 1879, I was summoned to Mrs. Nellie M., a primipara, aged 18. I found, lying upon a double couch, a small, delicate young woman having already strong bearing-down pains. Found the liquor amnii had already escaped, a well-dilated os, also a prolapsed cord and a cephalic presentation (vertex). Fully realizing the imminent danger to the child produced by the direct pressure of the cord between the presenting part and the walls of the pelvis (the child, in this instance, being still alive, as shown by the pulsation of the cord), I immediately changed the position of the woman by placing her, instead, on her hands and knees, with her hips elevated and her shoulders on a lower level, hoping thereby that, by a change of posture alone and a little manipulation, the cord would perhaps slip back by its own weight into the uterine cavity. To Prof. T. G. Thomas, of New York, I believe, belongs the credit of first calling attention to this postural treat-

ment, although, according to Prof. Playfair, of London, a somewhat similar plan had been occasionally adopted previously, but by whom he does not state.

The woman having been placed in the above position, but finding the cord had not slid back, I then gathered a long prolapsed loop of it between my fingers, and endeavored to replace it into the uterine cavity, behind the presenting part; but, after half an hour of careful, gentle manipulation, I found all my efforts to replace it were unavailing, the pains at this time being frequent, very strong and forcible. Simultaneously the genu-pectoral position had become so irksome to the woman that she begged to resume the recumbent position, which was granted, being mindful, however, of the necessity to place the funis as far as possible beyond the reach of dangerous pressure. A short time after, and while endeavoring to do this, I noticed that its pulsations had stopped entirely, indicating that the child was dead, and, of course, any further efforts of prevention or to replace it were unnecessary.

It may be proper to state here that, while endeavoring to replace the cord, the ends of my fingers came in contact with a hard substance, which proved to be the promontory of the sacrum. Suspecting that I had a case of contracted pelvis, I immediately decided to ascertain the length of the antero-posterior diameter. For this purpose I carried my right index finger, with its radial edge, against the curve of the pelvic arch, towards the promontory of the sacrum, and by applying the nail of the index finger of my other hand, at the point of contact of my right finger with the pubic arch, I was able to estimate the measurement of the inter-space, which, in this case, I found to be two inches and a half.

The indications now were to use or apply the forceps first, if possible, and failing to deliver with them, the next best thing was to perforate the head while the forceps were still on, compress the bones with these instruments, and renew the attempt to deliver. Recognizing the necessity for immediate delivery, I sent for my friend, Dr. Nathaniel Matson, but he being absent, his brother, Dr. William B. Matson, came instead. He examined the case, agreed in the diagnosis, and with me considered it best to manage or treat the case as above stated. The patient's bowels and bladder having been evacuated, she was placed under chloroform, brought to the edge of the couch and placed in the so-called lithotomy position, each limb being held by a woman. Hodge's forceps were now applied in the usual manner with very little difficulty. The handle of the male blade was left in the hands of an assistant sitting on my right. Holding, next, the female blade in my right hand, it was carried in a nearly perpendicular direction towards the left groin, and its lower point slid along the palm of my left hand, and, guided by it, in the direction of

the axis of the vagina, the inferior and superior straits. During this movement, it must be remembered, the handle is correspondingly depressed till it comes in contact with, and crosses obliquely the blade first introduced; in this instance, however, I found it at first impossible to bring the points of junction together; I, therefore, was obliged to withdraw this blade and re-apply it as before, but even then, in order to accomplish this, both handles had to be well pressed back on the perineum. An attempt was now made to deliver the child, but, being ineffectual, it was considered best to perforate the head at once, and, leaving the forceps still on, I carried two fingers of my left hand as a guard and guide into the vagina, up to the most prominent part of the head, and, with the right, introduced Hodge's perforating scissors until the scalp was reached, and, fixing its point on a bony part (some recommend a suture or fontanelle), was pushed up to the shoulders of the blades, freely rotating them to break up the membranes and the pulpy mass of the brain. This accomplished, I withdrew them partly, and, introducing them again, this time, however, with one blade only entering the cranium, while the other was passed carefully between the scalp and bone, and, having divided portions of the parietals, the edges were turned in another direction, cutting again as before, till several other openings were made. The forceps being already applied to the head of the child, the handles were firmly grasped, and, to make the pressure still more firm, a strong fillet was wound around the handles. The head, under this pressure, readily yielded, or collapsed; it was thought that its diameter had sufficiently diminished to allow its descent into the pelvis, and its subsequent delivery without any difficulty. Using the forceps again as tractors, another attempt was made to deliver the head, during which Dr. Matson, coming to my assistance, repeated the attempt, but was also unsuccessful. Finally, it was decided to lay them aside as being insufficient; in fact, it was noticed that they had gradually slipped away from the head.

Having found it impossible to deliver the head, we resolved next to remove the whole of the vault of the cranium, hoping thereby to diminish its size to the greatest degree, leaving only the bones of the face proper and the base of the cranium. The parietal bones having previously been divided with the perforating scissors, the ends of each were now seized and twisted off in succession with Elliot's craniotomy forceps; the rest of the bones were removed in a similar manner—having taken great care not to wound the maternal tissues with the serrated fragments during their extraction. This was prevented by the fingers of my left hand previously introduced into the vagina. It must be stated that during this operation, whenever it was convenient, the craniotomy

forceps were also used as tractors; for this object one of the blades was introduced through the perforation, and the other in opposition to it on the outside of the scalp, and, by making downward traction, it was thought at one time the head was felt descending. In the mean time a slit in the scalp (probably made by the sharp edge of one of the bones) had been discovered, through which I managed to insert or hook two fingers of my right hand, and bringing them out through the perforation, found them to fulfill admirably well all the purposes of a crotchet or tractors, the action of which was further increased by the bearing-down pains of the woman herself. To effect this the chloroform was now discontinued, and with each pain traction with the fingers was made; in this manner, to our great relief and satisfaction, the delivery of the head was at last effected. Taking this now between my hands, an attempt was made to extract the body; but finding all efforts ineffectual, a blunt hook, guided by the left hand, was next applied under the left axilla, and making traction on it, the shoulder, after great difficulty, was also delivered. The same was done with the other shoulder; then placing a hand under each arm, it was thought no more difficulties would be encountered; but not until after some more downward traction was made did I succeed in delivering the rest of the body.

It is needless to go into any further details; sufficient to state that the placenta came away an hour after. To effect its expulsion, however, it was found necessary to use moderate pressure on the lower part of the abdomen over the fundus, aided by gentle, steady, gradual traction on the cord. Previously to this, and during this operation, a drachm of Squibb's Fluid Extract of Ergot had been administered to hasten uterine contractions, and, consequently, the expulsion of the placenta; also to prevent any hemorrhage. Just prior to leaving the patient, who at this time was feeling as comfortable as could be expected, it was considered best, for procuring sleep, and to mitigate or prevent, if possible, a too severe shock to her nervous system, to administer to her twenty drops of Magendie's solution, leaving another similar dose, with instructions to be given in three hours, if she was restless or unable to sleep. This, I learned the next morning, was also given. It may be interesting to state that the temperature of the patient for the first two days did not go above 100° F., and on the third day 99° F. only. The pulse, however, was rapid. Her recovery from this time was rapid and unmolested, nor retarded by any accident or complication.

REMARKS.—There are some points in this case upon which I wish to make a few remarks. Prof. Playfair, in his *System of Midwifery*, page 338, 1st edition, begins his chapter on deformities of the pelvis with the following words: "Deformities of the pelvis form one of the most im-

portant subjects of obstetric study, for from them arise some of the gravest difficulties and dangers connected with parturition. A knowledge, therefore, of their causes and effects, and of the best mode of detecting them, either during or before labor, is of paramount necessity." With a view to know the existing cause in the present case, I made a thorough investigation of the past history of the early days of her childhood; but her mother, to whom I addressed these questions, assured me that her daughter from the day she was born had always enjoyed good health; in fact, had not had any of the diseases common to childhood, except the whooping-cough, although, at the time of her confinement, her delicate looks and appearance led me to entertain a different opinion, probably due to her pregnant condition. My object, of course, was to ascertain, if possible, whether this woman, in her childhood, had ever been a subject to the diseases called Rachitis, or osteo-malacia, these two diseases being the two principal ones known to produce deformities of the pelvis.

"In another class of cases," says Prof. Playfair, "the ordinary shape is modified by weight and counter-pressure operating on a pelvis in which one or more of the articulations is ossified." Further on, he continues: "The first class of deformed pelvis to be considered is that in which the diameters are altered from the usual standard, without any definite distortion of the bones; and such are often mere congenital variations in size, for which no definite explanation can be given." Still further on, page 342, he says: "Diminution of the antero-posterior diameter is most frequently limited to the brim, and is by far the most common variety of pelvic deformity. In its slighter degrees it is not necessarily dependent on rickets, although, when more marked, it almost invariably is so. When unconnected with rickets, it probably can be traced to some injurious influence before the bones have ossified, such as increased pressure of the trunk from carrying weights in early childhood, and the like. By this means the sacrum is unduly depressed and projects forward, so as to slightly narrow the conjugate diameter."

There is no doubt that the present case belongs to this class, especially as the early history of it has failed to reveal the fact that the patient had ever been subject to rickets or osteo-malacia. Then again, in view of the fact that she is the daughter of a poor, hard-working woman, it is natural to presume that she, in her early childhood, had been exposed to such causes as the ones above alluded to, and in this manner only became the victim of her deformed condition.

It will, perhaps, have been noticed that Prof. Playfair, in speaking of the pressure of the trunk by the carrying of weights, etc., leads us to infer that the narrowness or diminution of the conjugate diameter which is

found in such cases, is only slight, but whether this occurs in all cases cannot be accepted as altogether the rule ; at all events, there are exceptions to it, as has been shown in the present instance, where, instead of a slight diminution of the antero-posterior diameter, a great difference was observed, verified by the difficulties encountered during the extraction of the child.

As to the frequency and causes of prolapsus of the umbilical cord, Prof. Playfair, considered one of the latest authority, in page 299, says : " Fortunately, it is not a very frequent occurrence. Churchill calculates that out of over 105,000 deliveries, it was met with once in 254. Its frequency varies much under different circumstances and in different places." Then he continues quoting from the same tabular figures presented by Churchill and others : " We find a remarkable difference in the proportional number of cases observed in France, England and Germany, viz. : 1 in 446½, 1 in 207½ and 1 in 156, respectively." Further on, we find still a more remarkable difference observed in Germany itself, where the proportion in certain districts has been found exceeded, according to Engelman, whose records in the Lying-in Hospital at Berlin gives 1 out of 94, and Michaelis 1 in 90 in that of Kiel.

If the above figures be accepted, some convincing explanation must be given to account for this remarkable difference that has been observed between these different countries. Thus Doctor Simpson is known to have been the first to suggest very ingeniously the theory that the difference in frequency may depend in the varying position in which lying-in women are placed during labor in each country. This hypothesis is based on the fact that in France, where the woman is laid on her back and the pelvis elevated, the complication occurs least frequently. In England, where she is laid on her side, more often; while in Germany, where she is placed on her back with the shoulders raised, most often. Others have been suggested, however, which I think can be accepted as being the most tenable and correct. I am inclined to believe that the difference in frequency depends altogether upon the real cause of the accident, rather than the chosen position the woman has been placed in. Engelman, it will be seen, has given us a different view or explanation; one, I think, more acceptable and worthy of further consideration. According to Prof. Playfair, from whom I have quoted this, says: " The special frequency of prolapsed funis in certain districts, as in Kiel, is supposed by Engelman to depend on the prevalence of rickets, and, consequently, of deformed pelvis, which, we shall presently see, is probably one of the most frequent and important causes of the accident."

This leads me to speak of the causes of these complications. In three

cases I have already met with during my practice, I found the cords, by measurement, of an unusual length. In the present case, the cord extended in length three feet and three inches. This, as we all know, is one of the principal causes of this accident. On the other hand, Engelman says: "Prof. Playfair attaches great importance to slight contraction of the pelvis, and states that in the Berlin Lying-in Hospital, where accurate measurements of the pelvis were taken in all cases, it was almost invariably found to exist." "The explanation," says Prof. Playfair, "is evident, since one of the first results of pelvic contraction is to prevent the ready engagement of the presenting part in the pelvic brim." The present case, I think, has illustrated and corroborated the above statement, although we must admit, also, the unusual length of the cord. In conclusion, I have little more to say, except a few words on the cephalotribe and version. Had I foreseen or anticipated the difficulties above related, or should I, in the future, meet with another such complication, I would at once resort to the cephalotribe (having tried the forceps first), for I believe that, with this instrument, I could have crushed in less time and to the greatest degree, not only the vault, but also the base of the cranium and bones of the face simultaneously, without exposing the maternal structures to the well-known dangers of craniotomy, although I doubt even then, whether the result would be better, or the success as good and happy as in the present case.

As regards version, it was considered impracticable, if not unjustifiable, in this case, from the fact that any attempt to turn or change the position of the foetus after the liquor amnii has been long evacuated, is not an easy task, and I doubt very much whether it can be accomplished at all, without some risk of injury to the maternal structures; in such cases I consider the forceps or cephalotribe, and even craniotomy, preferable and safer; certain it is, that in the present case we were justified in not doing it, as shown by the difficulties encountered in the delivery of the head.

NASO-PHARYNGEAL CATARRH.

BY J. H. STERLING, M.D.

Should naso-pharyngeal catarrh, as a disease, demand our closest observation and attention, or are its worst results found only in its demoralizing influences on the system generally?

I have given considerable attention to diseases involving this locality, which has led me to inquire regarding the diagnosis, complications, prognosis and treatment.

It has been a surprise to see the number of persons who present themselves for treatment, not because they care anything about the catarrh, but because they are afraid it will finally lead to lung troubles. Insure them this will never follow, and they very soon begin to grow careless about their catarrhal troubles.

Just here is where we stand in a very serious relation to patients who seek our advice, and who will be guided, in a large measure, by that advice. We should not be guilty of making their case a very serious one, leading patients to believe there was nothing left for them but a steady and sure course to the grave; neither should we be guilty of lulling them into a state of repose and carelessness regarding their true condition. We are not infallible; we may often make grave mistakes, but, if possible, we should always try and err on the right side. We have fashions in medicines, and I am afraid it is the fashion generally adopted to tell those suffering from this disease: Well, if it gets any worse, I will try and do something for you; but, while it is causing you no serious inconvenience, I should leave it alone, for there is a strong probability it can never be cured.

A hasty glance at a few of the symptoms of this disease will suffice:

The voice is generally somewhat interfered with, its resonant quality is lost; speaks through his nose is the way they generally describe it; but the fact is, this is just what he does not do, because of the impairment of nasal respiration; breathes with mouth open, hacking cough, or rather a scraping of the throat, or continually trying to draw the mucus back into the throat, so it may be discharged; this condition is much influenced by the atmosphere. Hearing may be somewhat impaired; seldom much pain, except from complications, then we may have severe pain over the brows, through the face, radiating from the nose; in fact, I have one patient who has pains almost all over him, through his head,

chest, down his back, over the hips, loss of appetite and flesh, cannot lie down at night ; as soon as I can relieve his catarrh, all these pains and difficulties leave him ; but let him get cold, or through negligence his catarrh gets worse, and all his symptoms return. There is also a sense of fullness deeply seated in the back of the nose, a stinging, tingling sensation about the uvula and soft palate, or, as a lady described it to me, her nose and throat felt hot and peppery.

Some patients will have all these symptoms and many more ; other patients will have very few symptoms beyond a feeling of fullness in the nasal region, and that desire to keep drawing the mucus down into the throat. The catarrh may have existed for months or years, and he become so accustomed to his condition that he is hardly conscious of his real suffering ; the only correct and positive opinion is gained by ocular inspection and with the aid of the rhinoscopic mirror. This requires some experience coupled with patience on the part of both physician and patient ; but if we are careful, patient, and use some little dexterity in handling both patient and mirror, we will probably succeed with the majority. Thus aided, we are better able to make a correct diagnosis and have some foundation for rational treatment. It is as necessary that a correct diagnosis be made in these cases for treatment, to be successful, as in any other disease.

To give in detail what is to be seen on such an examination would take more time than the limits of this paper will allow, so I refer you to any of the text-books on diseases of the throat.

I pass over its pathology and take up a few of the complications that may follow this disease. Inflammation of the naso-pharyngeal spaces may extend to all the cavities adjacent to them and there set up severe inflammation which is very hard to reach. One complication, that is, middle ear troubles, no doubt very often starts from a naso-pharyngeal inflammation ; and I am warranted in the assertion that many cases of deafness in old persons is due to a chronic inflammation of the naso-pharyngeal spaces, from here stealing its way up the eustachian tubes, involving them in the same slow chronic process, and finally involving the middle ear ; its progress being so slow and mild in character, is at first hardly perceived, but finally resulting in almost complete loss of hearing.

We have hastily passed over its upward progress, what about its downward progress ? does it ever pass downward, or is its progress always onward and upward ?

Some take this view, while others contend that it does pass downward, involving all the different localities until the lungs are reached ; if this is the case, then this is the complication that demands the most atten-

tion. We know that bad cases of pharyngitis will extend downward to the larynx, and that chronic laryngitis is frequently complicated with inflammation and ulceration in the trachea, but possibly it spends its force here, and extends no further.

Ziemssen's *Cyclopædia*, on coryza, says the inflammation extending backwards leads to pharyngitis, and that pharyngeal catarrh may extend further down the respiratory tract to the larynx, the trachea and bronchi.

We are all aware that patients frequently first complain of a cold in the head for a few days, then develop a sharp attack of acute bronchitis. If these acute inflammations pass from one locality to another until the bronchi are effected, why cannot a chronic inflammation travel the same road, especially if the person has a strong tendency to inflammation of mucus membrane, or an hereditary tendency to phthisis? In Ziemssen's *Cyclopædia* we find that, "Inasmuch as several conditions generally concur for the production of chronic diseases, the physician will often have to decide for himself whether, and how far, the question of inheritability enters into the problem. I believe a systematic and persistent observation of judicious rules of life may overcome such an inherited tendency, but I have seen this fortunate result only in rare instances. Usually, in such persons, one or several parts of the body are again and again the seat of disease and form the locus minoris resistentia. If this be situated in the respiratory organs, frequent and long continued catarrh of the nose, larynx and air passages occur, and perhaps give rise, ultimately, to pulmonary disease, which develops in phthisis." He does not make a positive statement, but is inclined to the belief that a neglected catarrh, under certain conditions, may finally end in phthisis.

Prof. Loomis, of New York, says clinical experience establishes the fact that a large proportion of cases of catarrhal and fibrous phthisis begins with a naso-pharyngeal catarrh, which gradually extends to the larger bronchi, and then to the bronchiole, and, as has already been shown, finally develops lobular pneumonia, which leads to phthisical developments. Why an apparently simple catarrh leads to the development of phthisis in one case, and not in another, can be readily explained by reference to the general cause of the disease already referred to.

Prof. Armor, in a paper read before this Society nearly a year ago, advocates this same theory, and says, with the catarrhal form of phthisis there can be no doubt of this relation; no fact is better established in the clinical history of the disease. A large proportion of cases of catarrhal phthisis begin with a naso-pharyngeal catarrh. The catarrhal condition extends to the bronchioles, developing in one case lobular pneumonia, with all that that implies, in its relation to phthisis, and in another to

peri-bronchitis, which usually accompanies the fibrous forms of the disease.

This statement is certainly a very strong one, and, if true, certainly demands attention, when we consider how many fall victims to this disease every year. If the starting-point is in the naso-pharyngeal spaces, it is to this point we should direct our closest attention, that we may, if possible, arrest it in its very start. I have but little doubt that many cases of phthisis have their starting-point in this locality; but that it extends, as an inflammation, from point to point, until the bronchioles are reached, I think, in the majority of cases, is a doubtful question.

It is very certain there is a wide difference of opinion regarding this point. But why should we so differ? One says, truly, the patient had naso-pharyngeal catarrh for a long time, and finally developed phthisis; but, upon examination, the larynx and trachea were not involved in any inflammatory process whatever, which, they say, completely explodes the theory of extension.

Is there any way of explaining these cases? I think there is. In some cases this gradual extension may take place; but, in a majority of cases, it has another and more direct medium of communication, through respiration. There is but little doubt you may have a catarrh for years and still have a healthy pair of lungs. You may be exposed for years to all the different changes our variable climate subjects you to, and suffer from catarrh, bronchitis, pleurisy, pneumonia, and recover from them all, and finally die from some other disease. Further, we cannot safely say all stone-cutters, scissor-grinders, or persons whose occupations compel them to breathe an atmosphere loaded with irritating substances, die from phthisis; still, we do know that such a disease exists, and a certain proportion of persons following those trades die from phthisis. I am convinced that naso-pharyngeal catarrh, as well as other diseases of the nose which interfere with our breathing through it, may be the starting-point of phthisis, provided the patient is a good subject. We well know how hard it is to keep some patients from developing phthisis, while it would be almost as hard to make others develop it. Any cause which compels us to breathe through the mouth is unnatural. Healthy respiration should be carried on through the nostrils, that the temperature of the air breathed may be modified, made warmer, moistened and filtered of dust and dirt that is continually floating in the air we breathe. If from any cause a person cannot breathe through his nostrils, the air will enter the lungs cold, dry, and loaded with particles of dust, and whatever other irritating substances may be floating in it. This irritation, no doubt, in many cases finally causes inflammation, which extends from the bronchi to the bronchioles, finally ending in phthisis.

The question naturally arises, if it gives rise to irritation and inflammation in one case, why not in all? We can only answer this question as we have to answer many questions of the same nature, regarding other organs in the body, that, in some cases, will seem to tolerate any amount of irritation, while in other cases the smallest amount of irritation will often produce the most disastrous results. In a general way, therefore, the adage is correct, that, in the case of strong constitution, without hereditary tendency or taint of any kind, the disposition or liability of slight irritation setting up inflammation is diminished; but it cannot be ignored that considerable fluctuation occurs within these limits.

In this way naso-pharyngeal catarrh may cause phthisis, not by extension, but by interfering with natural respiration, and irritation caused by irritating substances passing into the lungs. Neimeyer says, a proof of the extremely important rôle played by the presence of foreign substances in the air passages, as an exciting cause of consumption, is found in the great prevalence of the malady among operatives and other persons who constantly live in a dusty atmosphere, such as stone-cutters, file-grinders, hatters, wool-carders, cigar makers, etc.

Prof. Loomis says, such admixtures of dust and air as are present in workshops and large manufactories are prolific exciting causes of phthisis. The most dangerous occupations, as regards the development of phthisis, are stone-cutting, knife-grinding, steel-polishing, diamond-cutting, etc. The constant inhalation of noxious gases, such as are generated in an overcrowded, badly ventilated apartment, is another very important element in the production of phthisis.

We might go on citing evidence from other authors, but the point I should like to bring more fully to your notice is, they accuse certain trades, badly ventilated, dusty rooms, as being a prolific source of phthisis, but say nothing of the condition we are all constantly subjected to, that is, an atmosphere which, in our large and very dirty cities, is constantly loaded with dust and dirt of the filthiest nature. Take, for instance, a person suffering from naso-pharyngeal catarrh, or any affection of the nose which interferes with normal respiration; the air enters the lungs through the mouth, and naturally escapes the filtering process it should have been subjected to. I should like to ask the question, what is the difference between this man's situation and one whose calling compels him to be in a badly-ventilated dusty room? The only difference is in degree. It may take a little longer to develop the disease in the first instance, but I think it is as sure to come.

I do not wish to be understood as saying that all persons suffering from naso-pharyngeal catarrh finally develop phthisis; for we know that

many persons suffer from this disease and still breathe partially through the nose, or at times will breathe through the nose as well as ever. But what I do mean to say is, that a person suffering from catarrh, compelled to breathe with his mouth open, at the same time inherits a strong tendency to phthisis, and perhaps living under bad hygienic influences, is very liable to develop phthisis, not by the inflammation extending to the lungs, although this may be the case at times, but by the particles of dust and other irritating substances entering directly into the lungs and setting up inflammation.

It has been my privilege to watch a very interesting case for some eighteen months. Her occupation compels her at certain seasons of the year to be much exposed to an atmosphere loaded with particles of dust, dyes and very fine fibres of a woolly nature; after thus being exposed for a time, she would begin to suffer from catarrh. Upon examination with the rhinoscopic-mirror, the mucus membrane would be seen thickly coated with mucus mixed with dust, dye, etc., which had been floating in the atmosphere; her nasal passages were quite large, and at first respiration would be but little interfered with, but in a few days she was generally compelled to breathe through the mouth; if at this period she still continued her work, she was sure to have quite a severe attack of bronchitis, with pains through the chest, etc. I examined this case repeatedly, but could not detect any laryngeal inflammation, as if it had extended from above downwards. It seems to me a very clear case, that as soon as she was compelled to breathe through the mouth, the air was no longer warmed, moistened and filtered, and the result, inflammation due to irritation.

TREATMENT.—If naso-pharyngeal catarrh is a disease which is often followed by so serious a complication as phthisis, it certainly demands attention from physicians, regarding its treatment. At present, judging from the number of patent medicines sold for the cure of this disease, its treatment is largely in the hands of those who make a business of manufacturing these medicines. This is little to be wondered at, when we know that physicians give so little attention to this disease. Now, often patients are given something to gargle their throat with, and told to call again if they get no better; the patient gets disgusted, and the disease is generally left to run its own course. Naso-pharyngeal catarrh can be treated successfully, but much depends upon the amount of care we give each case; for much depends upon the local treatment, although we should never lose sight of any systemic disorder. Each and every case demands its own treatment.

It requires as much care and attention to treat a bad case of naso-pharyngeal catarrh as it does a catarrhal inflammation of the neck or

body of the uterus; and who would think of trying to cure a chronic inflammation in this locality by constitutional treatment? The constitutional and local may be combined, with much better result than when only one was used. Who would think of treating a chronic inflammation of the neck or body of the uterus by merely injecting some astringent solution against the os? We certainly could never expect to be successful; but there is no doubt the treatment would be just as rational as to expect a gargle to cure a case of chronic naso-pharyngeal catarrh.

The first and most important point in the treatment of this disease is cleanliness. All diseased mucus membranes, wherever found, if covered with secretion, must be thoroughly cleansed before any medicinal agent is applied. Upon this point depends our success in its treatment. If we are only going to do one of the two things, choose the first. These cases will do better if we thoroughly cleanse, and make no application, than they will if we make the most skillful application on top of the thick, tenacious mucus and filth that generally covers those surfaces. Always examine them after the cleansing process, to see if everything is completely washed away; for we will often be much surprised to find the surfaces still covered with a thick, sticky coating, when we thought it should be perfectly clean. Knowing that alkaline solutions have a solvent effect on mucus, I always use some alkaline solution. The one generally preferred is Dobell's Solution. It is simple and quite effective. The mode of cleansing is of some moment. I generally use the atomizer or post-nasal syringe, sometimes both. In some cases nothing will dislodge the mucus but a probe, armed with cotton. One or all these different means may be used. Their only object is to remove the secretions without any injury to the adjacent parts, or mucus membrane beneath. If I have impressed upon you the necessity of a thorough cleansing, I have said all that is really necessary about the treatment of this disease.

The medicinal agents to be used are numerous, and must be chosen from quite a large number to suit the individual case. They may be in the form of solution, powder, ointment or spray.

I am very fond of using the spray, as it reaches cavities almost inaccessible to other modes of treatment. The astringents used must be of different strength to suit each case. We may select from the different preparations of zinc, ferric alum, tannin, nitrate of silver, etc. As a standard solution, I generally use the chloride or sulphate of zinc, but prefer the chloride. When there is hypertrophy we must use strong applications. There is one agent that is but little used, and that is iodoform; but it is of decided benefit in many cases, especially in cases with hypertrophy and considerable irritation. It relieves the irritation, and reduces

the hypertrophy about the turbinated bone, better than any agent I have used. Its great drawback is its smell, which clings to the patient for days. It can be used in solution, powder or ointment; the last I think the best. In the atrophic form, the glands need to be stimulated to action. Weak solutions of iodine, glycerine and water, or inhalation of steam, is often of service; or the internal use of remedies which excite the secretions from mucus membranes, such as cubebs, muriate of ammonia, etc., or what I think better, a very weak current of electricity. I have used electricity in other varieties of catarrh, and consider it a very useful agent. There are many other agents we have to resort to for the cure of this disease; but, like every other disease, when one thing fails—as all old remedies will at times—we have to hunt up something new, and we might go on bringing up different remedies without limit.

One word about the application of remedies. There are all kinds of instruments and brushes made for this purpose. The one I like the best is a simple uterine probe. You can bend it into any shape, to suit yourself or the locality you are going to use it in. Another advantage is, it bends easily, and you cannot use enough force to do much injury to the tissues. We are often tempted to use a little more force than is desirable; but, with this instrument, there is but little danger of any such accident.

DISCUSSION.

DR. S. SHERWELL did not believe that naso-pharyngeal catarrh had ever caused a case of tuberculosis—at least he had never seen one. He agreed with the reader, however, that it will help to produce phthisis; but not that naso-pharyngeal catarrh is an essential factor in its production, or that it leads directly to phthisis. He did not think it is so claimed by any laryngologist, and certainly they ought to know something about it.

DR. J. H. H. BURGE referred to the statement that a vast majority of cases of this class are in the hands of quacks. It reminded him that one of the most active ingredients in one of the most popular nostrums for the cure of this disease is salicylic acid. He had no doubt but that its influence on the disease was good. He had tried it himself with benefit.

DR. SHERWELL was of the opinion that the chief nostrum of this character contained principally nitrate of silver.

DR. BURGE thought not—at least the nostrum to which he referred contained none. He could readily distinguish between salicylic acid and nitrate of silver. The skin about the nose was not turned black by the action of light.

DR. W. H. BENNETT fully agreed with the author of this paper as regards the influence of dust in the production of naso-pharyngeal catarrh and phthisis. He referred not to tuberculosis, but to catarrhal and fibroid phthisis. In the year 1872 he called the attention of the profession to the influence of street dust in the production of disease. In making laryngoscopical examinations he had noticed dust on the surface of ulcers in the larynx and trachea forty-eight hours after exposure. He had noticed the same circumstance in regard to the vomer, when making rhinoscopic examinations. There can be no doubt of the fact that dust caused catarrhal and fibroid phthisis. Catarrh

affects the lungs in those persons suffering therefrom who breathe the air which passes over putrescent nasal secretions, and they must necessarily experience the evil result. The speaker had experimented on the subject of dust as a causation of disease. He had confined animals in a room in which particles of dust were caused to float in the air, and none lived beyond twelve months. It seemed to him that catarrh might be divided into different varieties. 1st. That variety in which there is an abundance of secretion. 2d. A variety in which there is scarcely any secretion; there appears to be simply a cell proliferation; and 3d. A variety in which the glandular apparatus is more or less involved. Almost all cases of chronic catarrh become, sooner or later, cases of proliferous disease, the secretion becomes diminished, and then hypertrophy of the infiltrated cells appears. In regard to the treatment, he had only to say that, in his estimation, it is a subject which demands more attention than it ordinarily receives.

DR. A. MATHEWSON: The treatment of the air passages consists largely in the treatment of the pharynx. He did not accomplish very much if he did not take this fact into consideration. Fully 75 per cent. of catarrhal cases involve the pharynx. As a practical point, in the application of fluid remedies to these parts, it is well to direct the patient *not* to blow the nose, or cough, for a short time after the application. Cases are quite common in which severe irritation or inflammation of the middle ear results, the fluid being forced up through the eustachian tube by the expulsive efforts above referred to. Even snuffing so simple a mixture as salt and water is sometimes quite as bad as blowing the nose. He is always very careful, after applying a fluid, to direct the patient to wait some time before blowing the nose.

DR. E. R. SQUIBB referred to experiments now being made in France with dust, the object being chiefly to discover bacteria. So far as he knew, bacteria are not found in nasal and buccal secretions. A nice way to filter the air is to fasten a piece of dampened sponge before the nostrils. A person using this apparatus (which is largely used in pharmaceutical manufactories, while working in substances of a more or less dangerous nature, such as aconite root, and the like) will find an abundant collection of dust on the sponge, for the reticulation of the sponge is very great, and will prevent dust from entering the air passages about as effectually as almost any other substance.

DR. J. WALKER spoke of overheated houses as a prolific source of catarrh. Persons thus overheated will incautiously expose themselves to a colder temperature. A very common cause of catarrh is the dust which arises from and through old, dusty, rusty furnace flues. Rusty furnaces, becoming so through the summer, are especially bad in this regard. Then, again, the air chambers of furnaces do not, in all cases, communicate with the open air. They open into the cellar, and take up the unhealthy air of cellars, with all its odors and poisons, as, for instance, from decayed vegetation and the like. He had in his mind a case where the air chamber of a furnace was two or three feet from the wall. The speaker knew of a row of houses built upon the same plan—which is a dangerous one, and ought to be remedied.

THE CHAIR said Dr. T. R. French refers, in a note, to the use of the Thudicum douche in the treatment of catarrh. He says: "I have used the douche in the treatment of chronic nasal catarrh and eczema in 165 cases, and have yet to see the first case of middle ear trouble, such as Dr. Roosa and other ear men tell us is apt to follow the use of this instrument. This I attribute largely to the care taken in directing the patient in its use. If care is not taken by the physician in directing, and by the patient in using the douche, I can understand how injury might be done to the ear. I use the Thudicum douche in my hospital clinic only; for, when my patients can afford it, I am in the habit of employing a spray douche, which has many advantages over the Thudicum douche." He asked Dr. Mathewson to state whether there was any greater danger in the use of this than other moist applications.

DR. MATHEWSON thought there was. He had reported several cases in connection with Dr. Roosa, where serious middle ear troubles had resulted from the use of the douche. One or two of these cases were physicians, who would be supposed to exercise great care in its use. One physician, who had taken part in a discussion of this subject, used the douche afterwards, but troubles of the middle ear was the consequence. So many cases have occurred in his practice, that his convictions are stronger and stronger that there is danger in the use of fluid applications if the patient blows the nose too soon after the operation. This is in consequence of the peculiar anatomical arrangement of the nasal air passages. The spray is not so apt to produce bad results. It would not be likely to send a current into the middle ear; or if it did, fluids in the form of spray are not so injurious as otherwise.

A NEW OBSTETRIC FORCEPS—MULTIPLE, ADJUSTABLE AND RE-ADJUSTABLE.

BY J. H. H. BURGE, M.D.

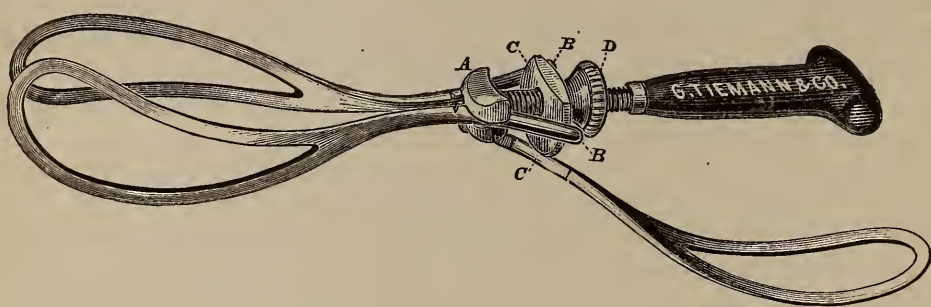
Since perfecting the instrument which I present to the profession this evening for the first time, I have, to some extent, reviewed the history of the Obstetric Forceps, and have taken some notes upon the subject which may not be void of interest to those of you whose thoughts have run in other channels.

It was my intention to read these notes to-night, but, upon reflection, it seems better to devote the few minutes at my disposal to the exhibition and description of my device, and to reserve the literature of the subject for another occasion.

If an alphabetical list were made of all the names which have been connected with modifications of the Obstetric Forceps, it would so nearly resemble a city directory that some apology seems necessary from one who seriously proposes to occupy your time and attention with one more novelty. I should hardly have the temerity to do it, if I were not supported, and even slightly elated, by the confidence that I have something useful to offer.

This instrument is so simple in its construction that if I had sent it to you last night in an obstetric emergency, by mistake for Elliot's, Bedford's or Smellie's, I believe you would have proceeded to deliver your patient at once, and without embarrassment; and yet I call it multiple. I give it this title, not simply because the blades and handle are made separate, but because the lock being in the end of the handle, you can

take your choice from as many blades as you please to carry, and they will all fit securely. If the convenience of carrying an instrument in the pocket when the handles are detached were a main feature of my device, I would have held my peace; for in this Pajot, of France, and Dr. F. H. Stewart, of this city, have preceded me; and the folding handles of Levy, of Copenhagen, Saxtorph and others are well known.



While upon this point, however, you will observe that as these blades do not cross, we have but one handle to carry. To describe this instrument, I have only to say, blades of any pattern you please. *These* are Elliot's, length, width, cranial and perineal curves, and all. These blades are separately slipped upon the foetal head, and when in place the handle is passed between them and drawn back until it locks at A. This lock forms a ginglymus, and is perfectly secure. The free extremities of the blades B B, projecting on either side of the handle, form levers by which the blades are separated or approximated at will. These levers are acted upon by a slide C C, which moves up and down freely upon the handle, and is secured at any desired point by means of a nut D D, which revolves also upon the handle. The amount of compression which the blades shall make upon the foetal head is under perfect control. The first peculiar advantage, then, which I believe this instrument possesses, is its perfect adjustability—you can make as little or as much compression as you desire; but what you make you make intelligently and intentionally. I tell you nothing new, gentlemen, when I say that in difficult cases there has always been some trouble in gauging the amount of compression at the same moment that great extractive force was necessary to be employed. Our own Elliot has testified to this difficulty by inventing, perhaps, the best of all the inefficient means employed to overcome it. I allude to the screw, which prevents a close approximation of the handles.

2d. This instrument is *re*-adjustable at any moment during the process of delivery, if an advance of the head or any change in its position relatively to the maternal parts should render such re-adjustment desira-

ble. The ease with which the handle is slipped on and off, the fact that the blades do not cross each other, and that they may be moved independently in any direction, and the additional fact that each blade may be separately manipulated and adjusted, give to this instrument an advantage which, I think, will be readily appreciated by the practical obstetrician.

3d. All blades being made to fit one handle, experts who desire to be thoroughly furnished for every emergency, can, at slight expense, have a sufficient variety—a multum in parvo—from which to select for immediate use. I regard it a peculiar excellence of this instrument, that it sacrifices none of the good things which have been given us, from the time of Chamberlain even until now. With it you may have the blades long, short, straight, curved, double-curved, fenestrated, non-fenestrated, etc.; in short, the accoucheur can suit his own taste, or follow his own convictions as to the form which he shall use. There is an absurdity in using the same instrument for all cases—for the delivering of babes weighing two pounds and those weighing fourteen, for one head absolutely spherical, and another extremely elongated. The ordinary long forceps has been brought to such perfection that its adaptation to these extremes is wonderful, and yet the great advantage to be gained by a proper selection can hardly be questioned. In the past this selection has been impossible.

4th. A notch or socket is provided on the under side of the handle for the attachment of a third blade, if, in any extraordinary case, it were thought desirable to use one. As this will seldom happen, the lock is made available for the *Tarnier* traction. A button on the lever end of the third blade fits this lock, and although it is not exactly in the line of the superior strait, it is sufficiently near it, in my opinion, to give us all the benefit of Tarnier's valuable suggestion. For the benefit of the few who are not familiar with the improvement of M. Tarnier, I will say that it consists of a bifurcated handle attached to the under surface (lower anterior edge) of the blades when in situ, by which traction may be made, as nearly as the perinæum will allow, in the line of the superior strait, at the same time that the ordinary handle is used. I will not enter now upon the discussion of this subject, my only desire being to call attention to the fact that Tarnier's attachment can be used with my instrument in his own way, or we may adopt the modification of it which I have described.

5th. As to facility of introduction, more depends upon the character of the case than upon the style of the instrument, and quite as much, also, upon the tact and experience of the hand that guides it. Nevertheless it will be patent to every one that a light, short blade

can be passed into the pelvis and upon the foetal head, *cacteris paribus*, more easily than the same blade with a handle of five inches attached. To apply the latter the thighs are often strongly abducted, and a prominent abdomen gives none too much room for the necessary manipulation. It has been suggested that with oily fingers one could not use these detached blades as easily as those with handles. A careful view of the construction of the instrument will show that, as soon as the blades are slipped approximately into place, they are, separately or together, more completely under the control of the accoucheur by means of the handle, which he can slip off and on as he pleases, than the blades of any other instrument—simply because they are manipulated separately, and do not cross and thus interfere with each other.

6. A straight blade is here provided to be used as a vectis. This can be slipped into place and the handle securely attached, almost without the patient's knowledge. The vectis is seldom used; but I believe that many cases of labor could be much shortened, and much suffering avoided by the judicious, tentative use of a single blade so entirely under the control of the operator.

7th. Last and least, and yet, I am sure, not unimportant, is the consideration that one can go into the presence of the patient without proclaiming the fact that the labor may become instrumental—a small, straight package, which would excite no suspicion, even if seen, being all that is required.

I desire now to anticipate a possible criticism which would be likely to fall from the first speaker, viz.: that it is unsafe and absolutely a move in the wrong direction, to seize a child's head in the dead gripe of an insensitive iron vise, and that blades operated by screws and levers must be this and nothing else. I have stated this objection as strongly as I could, because it is the first thought that would be likely to enter the mind of a stranger in reading a description of this instrument, and I hope to show to your entire satisfaction that, although it is strong, it is perfectly safe and manageable. You will observe: 1st, that the screw D, by the feeling of resistance which it imparts to the hand of the operator, gives a better indication of the amount of compression being made upon the head of the foetus than any accoucheur can get while making powerful traction with the ordinary forceps; and you will observe—2d. that the very instant you cease making traction, you can entirely relax the pressure by a single turn of the screw.

It has occurred to me that some of you may get the impression that I expect by this instrument to convert serious cases into simple ones. I desire to say distinctly that I have no such idea. If by its use the practitioner may be aided in his arduous task, and the sufferings of the par-

turient woman, in any class of cases, be relieved with greater facility, I shall be glad.

DISCUSSION.

DR. W. C. OTTERSON asked the doctor as to his practical experience with this instrument? Did it fulfill his expectations?

DR. BURGE replied that he had only just received the instrument from the maker, and had had no opportunity to put it to a practical test.

DR. J. S. WIGHT asked Dr. Burge what he intended to do with the blades of the forceps when a tremendous uterine pain came on; for instance: Does he intend to allow the blades to remain in the uterus? He referred to the danger of sharp-edged blades being allowed to remain in the uterus under such circumstances. He had often been tempted to file the edges of the forceps used by him (Elliot's), so that blunt, instead of sharp edges, will impinge against the soft parts.

DR. BURGE said the blades could be made of any desired length, and that each could be *separately* locked and manipulated after the introduction, if necessary.

DR. W. F. SANFORD thought Prof. Wight's remarks were very pertinent. It had always seemed to the speaker that the most troublesome part of the introduction of forceps is that of holding the first blade while the other is being applied. It requires considerable genius to hold the first blade at such a level that the blades may be locked without difficulty. He was inclined to believe there would be some difficulty in the practical working of Dr. Burge's instrument, because there is no handle to keep the first blade in position while the second is being introduced.

DR. BURGE replied that each blade is capable of being managed by the handle separately. The moment one blade is introduced, the handle can be put upon that, if desirable, until the second blade can be put in place.

DR. SANFORD said that that explanation put a different interpretation upon the matter.

DR. BURGE reminded the Society, in closing the debate, that all these remarks must, of necessity, be of a theoretical nature, as he had had no opportunity of practically testing his instrument.

—IN CONNECTION WITH DR. PROUT'S "Point in Hospital Management," published in last month's PROCEEDINGS, it is worth while to put on record the fact that the Rochester (N. Y.) City Hospital allows its private patients to choose their medical attendants from the list of reputable physicians in the city. A member of the staff cannot be compelled to attend a patient in a private room. The theory is that if a patient wants the luxury of a private room, he is able to pay for professional services. In Baltimore, Maryland, there are at least two hospitals conducted on the Carney Hospital plan. They are the "Church Home," an institution maintained by the Protestant Episcopal Churches of the city, and St. Vincent's Hospital, under the management of Sisters of Charity. This plan is said to work excellently well in Baltimore.

TRISMUS NEONATORUM—RECOVERY.

BY JOHN COOPER, M.D., M.R.C.S.E.

On January 20th, after a tedious labor of twenty-four hours, Mrs. M., aged 20 years (white), was delivered of her first-born mature female child.

At birth the funis was twice around the neck, which accounts for the delay in delivery; was in a state of asphyxia and exceedingly cyanotic. It took half an hour to resuscitate the infant. During that process a large quantity of frothy mucus, slightly tinged with blood, poured from the nostrils.

Eight hours after birth unilateral convulsions were observed of the left side. This was at 8 P. M.; twelve hours after it was general, with frequent spasms; rigid lower jaw, with mouth sufficiently open to admit a finger; difficult breathing, livid countenance, clenched hands, with thumbs flexed into the palms, and produced on the slightest motion, commencing with a little scream.

When seen, was immediately recognized as an old enemy that had not been witnessed by me for twenty-four years previous, who had vanquished me every time, and had hoped never to meet again. Have treated several cases among the negroes in Louisiana, but no means then used prevented a fatal termination. It has been stated that these cases "invariably occurred on the sea-coast, from cold and damp weather, and unknown in the interior of the country."

All the cases previous to this one were seen in warm weather, in the south, not less than two hundred miles from the sea-coast.

The surroundings in this case were all that could be desired—cold and dry, with thermometer about 32° Fahrenheit. I did not fail to warn all, except the mother, that they must prepare for a fatal result.

TREATMENT.—Thinking that it might relieve the brain, free catharsis was induced, with calomel and castor oil; after which five-grain doses of bromide of potassium in sweetened water; very soon observing no improvement, had recourse to the following recipe: Ext. physostigma, gr. ss., glycerine, ℥ij.; aqua, ℥vi. Dose, thirty drops every four hours. After three doses the convulsions were less severe, and after six had been given the paroxysms came on with much longer intervals, milder and of shorter duration; so that by the time eight doses had been given, they had entirely ceased and did not return, although the above was continued in half doses every six hours for the next twenty-four hours.

During this treatment the child was nourished with milk and barley water; although the feeding would induce convulsions, yet the child swallowed without difficulty. After the attack was overcome, for four or five days the child had not the power to nurse, so that Knapp's breast pump was used and fed to the child until she was able to help herself.

It was, a week after birth, as well and healthy as any child of that age.

It may be as well to state that particular directions were given during the attack that the child should be laid on its side, and not on the back, in order to avoid pressure on the occiput.

Feeling that I had a fearful case to deal with, was compelled without delay to make use of desperate means (many would consider the dose too large for a new-born infant) before the little patient was exhausted or became comatose.

Had the poisonous effect of the drug exhibited itself by tremulousness and loss of power of the extremities, becoming limp and flaccid, indicating the approach of general paralysis, should have used chloral as an antidote.

This is the first case of the kind in which I have seen the calabar bean used, and hoping it may prove as useful to others as it was in this, has induced me to report it.

VITAL STATISTICS FOR THE YEAR 1879.

BY R. M. WYCKOFF, M.D.

The actual mortality in Brooklyn during the year 1879 was 11,569. The total number of births reported was 10,169; of still-births, 889; of marriages, 3,222. Of these three classes the returns are incomplete, whereas the deaths are fully reported. Estimating the population at 564,448, the annual death-rate was 20.49 per 1,000 persons living; while in 1878 the rate was 20.40 per 1,000. This latter may be signalized as the lowest death-rate this city has experienced since our mortality statistics began to be fully registered. Next to that, however, stands the low death-rate of 1879; so that the health of the city, as measured by its roll of deaths, was considerably above the average of recent years. A comparison of the total mortality during certain years, and of the rates proportional to population, may be instituted by means of the following exhibit:

Years.	Deaths by All Causes.	Differences.	Population, (Census or Est.)	Death Rate per 1000.
1879.....	11,569	494 +	564,448 E.	20.49
1878.....	11,075	287 —	542,739 E.	20.40
1877.....	11,362	972 —	521,864 E.	21.77
1876.....	12,334	136 —	501,792 E.	24.58
1875.....	12,470		482,493 C.	25.84
Average 10 years.....	11,320		455,000 E.	23.78

There were no deaths by small-pox, and no serious epidemic prevailed. Yellow fever occasioned two deaths (both imported cases), but it secured no foothold. Diphtheria caused 689 deaths, an increase as compared with the previous year. Scarlet fever, 344, a falling off as compared with 1878. The number of deaths by certain other of the principal causes was: by whooping-cough, 204; measles, 40; croup, 250; typhoid fever, 59; cholera infantum, 680; all diarrhoeal diseases, 1,258; all diarrhoeal diseases under five years of age, 1,076; malarial fevers, 150; erysipelas, 64; intemperance, 67; rheumatism, 67; cancer, 231; marasmus, 353; consumption, 1,665; hydrocephalus and tubercular meningitis, 189; meningitis and acute diseases of brain, 372; apoplexy, 229; convulsions, 241; all diseases of the nervous system, 1,238; diseases affecting the heart, 505; bronchitis, 479; pneumonia, 975; all diseases of the respiratory system, 1,632; Bright's disease, 256; puerperal diseases, 182; old age, 239; infantile asthenia and premature birth, 257; suicide, 37; in public institutions, 637.

By the five principal classes of causes of death, the number of decedents was: I. Zymotic diseases, 3,283; II. Constitutional, 2,574; III. Local, 4,379; IV. Developmental, 1,017; and V. Violence, 316. By seasons, the record was: For the first quarter, 2,832; second, 2,478; third, 3,398; and fourth, 2,861. By sexes: Males, 5,823; females, 5,746. By nativity: born in the United States, 8,341; foreign born, 3,228. Under 1 year of age there were 2,881 deaths; under 5 years of age, 5,201; and at 60 years of age, and upwards, 1,684.

—FRACTURE OF RIBS.—Dr. H. A. Martin, the Boston surgeon who introduced the use of the pure rubber bandage in skin affections, reports that this bandage is an admirable dressing for broken ribs.

BROOKLYN PATHOLOGICAL SOCIETY.

Regular Meeting, Dec. 26th, 1879.

The President, Dr. F. W. Rockwell, in the chair.

MALIGNANT LYMPHOMA.

Dr. Westbrook presented microscopic specimens from a case which had occurred in the practice of Dr. D. A. Dodge.

History: Dr. Dodge first saw him professionally on the 1st of February, 1877, when "he had marked tertian symptoms: enlarged liver, complete anorexia, a feeling of constriction about the upper part of abdomen, impeding respiration, and extreme mental depression. These symptoms soon followed by a slight icterus, afterward changing to a bronze hue; clay-colored stools. Improved slowly and slightly, until May, when he sailed for the south of France, where he gained slightly until August, when he was, without warning, seized with an alarming hæmatemesis, which almost exsanguinated him. From that time he never recovered his color, but was of a waxy hue. Subsequently he had two or three attacks of hemorrhage, and returned home in June of this year (1879), being two years and four months from the beginning of his sickness. He then had several enlarged glands in the inguinal region, and, as he progressed to his end, fresh glands showed themselves in every part of the body. The post cervical glands and epitrochlear glands showed notably. The spleen gradually showed itself more and more. The bowels continued astonishingly regular, and only once or twice, for a day or two, did the color of the fæces deviate from the natural. Swallowing finally became impossible, and he died of anorexia. There was a history of syphilis twenty years before, but Dr. Van Buren, who saw the case with me, was positive that it had nothing to do with the disease of which he died. I may add that he was an importer of French wines, a free liver, but far from intemperate. His father died of cancer of the stomach."

The autopsy was made by Drs. Harvey and Geo. R. Westbrook. The *lymphatic glands* were everywhere enormously enlarged, forming tumors in the inguinal and axillary regions. In the *thorax*, they compressed the trachea and oesophagus. The *liver* was not markedly enlarged, but contained innumerable white spots and stria following the course of the vessels. The *spleen* weighed 90 oz., was dark red, and filled with white masses, being in size from a pin's head to a pigeon's egg. Microscopically, the appearance was that of lymphadenoma. The blood was not examined.

EXTREME DEGREE OF FATTY DEGENERATION.

Dr. Westbrook presented a piece of a liver which had undergone extreme fatty degeneration, together with a microscopic preparation illustrating the histology of the same. The patient, an Irish woman about 50 years old, was admitted to his ward in St. Peter's Hospital, complaining of debility and cough. She was quite obese, and the fat was soft and flabby. Her countenance had the red color of habitual drunkenness. The alimentary canal was not notably deranged. Physical exploration showed a diminished elasticity of the lungs. The heart sounds were feeble, the pulse exceedingly weak, regular and varying at about 100 per minute. She had a small amount of whisky, and a tonic mixture. On the second day after admission she became delirious, having delusions and hallucinations. Twenty grains of chloral with as much bromide of potassium were given at night, the whisky increased, and a dose of *tr. nux vomica* with Huxham's tincture, administered every three hours. She slept better the following night and had less delirium. On the third day the *nux vomica*, etc., were administered during the day, but at 11 o'clock in the evening she died very quietly. No chloral was administered that day.

Post-mortem.: The *heart* was found to be exceedingly flabby, light colored and friable. The *lungs* somewhat emphysematous. The *liver* of a light yellow color, and of such slight sp. gr. that pieces of it floated on water. The *kidneys* were similarly degenerated. The heart was not opened. Microscopically the hepatic cells were so distended with fat that the section resembled adipose tissue.

ENTERIC FEVER.

Dr. Westbrook presented portions of the intestine showing the typhoid ulcerations from a patient who had died in St. Peter's Hospital. The point of interest about the case was the profuse sweating which had occurred almost daily throughout the period of her sickness—about four weeks—and which had led to the suspicion that some pulmonary or other complication existed which he was unable to diagnosticate. The autopsy, however, revealed nothing more than usual.

The liver and kidneys showed acute albuminoid changes, and the spleen was enlarged to twice the normal size.

LIPOMA.

Dr. F. S. Stuart exhibited a tumor removed by Dr. A. J. C. Skene on November 30th, 1879, from Mrs. L. It had been first noticed about two years ago. "It was found to be imbedded in the under surface of the external oblique muscle on the left side, close to the

crest of the ilium. Before the operation it could not be distinctly outlined, as it was close to the ilium, and readily pressed into the fossa. The operation was performed under a spray of carbolic acid, and the wound dressed after the manner of Mr. Lister. The primary incision was parallel with the crest of the ilium, but, when it was found to be below the external oblique, the fibres of the latter muscle were separated as much as possible, rather than cut, and so the tumor removed. Its longer axis was parallel with the crest of the ilium. Union took place without any supuration, and the patient was dismissed at the end of a week.

“The tumor was removed because it was growing, and would, in time, have been more difficult to manage.” It was made up almost entirely of yellow fat, with very little connective tissue.

Regular Meeting, January 8th, 1880.

The President, Dr. Rockwell, in the chair.

GREAT HYPERTROPHY OF HEART—MITRAL STENOSIS WITHOUT ACCOMPANYING MURMUR.

Dr. Wm. Wallace presented this specimen. The following history is contributed by Dr. E. J. Wilson, House Physician to the Long Island College Hospital.

The patient, a Norwegian sailor, 21 years old, gave a history of rheumatism which occurred about two years ago. At that time he fell from the ship into the water, and was subsequently confined to bed. He says he experienced much pain when he breathed hard, walked or coughed; the pain referred to the præcordial region. Since that time he has experienced much inconvenience from shortness of breath, particularly when climbing aloft, ascending stairs or engaging in any brisk exercise.

PHYSICAL EXAMINATION—INSPECTION:—Body not well nourished and anæmic; prolabia pale; chest smooth and full; pulsation of carotid and subclavian arteries plainly visible. Impulse of apex seen in 6th interspace one inch to the left of nipple. *Palpation* negative. *Percussion*:—Area of cardiac dullness greatly increased, extending $1\frac{1}{2}$ inch to right of sternum in 3d interspace; inferiorly to 6th int., and in the left 3d interspace 3 inches from the border of the sternum. *Auscultation*:—A murmur occurring with first sound of heart, heard with maximum intensity in 3d right intercostal space close to the sternum, and transmitted upwards into the carotids. Also feebly heard in the interscapular region close to the spine on the *right* side. The murmur was soft and obscured the first sound. Another murmur heard in diastole with greatest intensity at the ensiform appendix. The urine was *acid*; sp. gr. 1025. Heat throws down a small amount of albumen; casts of a fatty and granu-

lar character. On the 14th of October sub-crepitant râles were heard at the base of both lungs. Nov. 21st, suffering greatly with pain referred to ensiform appendix and also a very severe headache. Heart's action so tumultuous that auscultation was unsatisfactory. Temp. 103° F.; pulse, 130; resp. 33. Nov. 23d, *crepitant* râles heard in right lung at base over the posterior and lateral aspects. Temp. 102° F.; pulse 124; resp. 36. Since Jan. 1st, dyspnœa amounting to orthopnœa has been almost constant.

Autopsy. The heart weighed 34 oz.; thickness of wall of left ventricle $\frac{7}{8}$ in. The left auriculo-ventricular opening was only large enough to admit one finger. The mitral valve was not present as a movable curtain. The aortic semilunar valves were covered with vegetations, some of which were calcified. The vegetations extended about an inch on the anterior wall of the ventricle. The valves of the right heart normal.

Dr. Wallace saw the patient first only two or three days before death. There was then the double murmur described above. He could hear no murmur at the apex. In reply to Dr. Pilcher, he said that he had not before seen the papillomatous growth extend into the cavity of the ventricle.

Dr. Segur had never seen the extension of the vegetations into the cavity of the ventricle.

ULCER OF STOMACH, WITH PERFORATION.

By Dr. Pilcher: "A. B., male, aged 56 years, suffering for some years from mitral stenosis; his sufferings from cardiac disease had become so extreme as to cause expectation of speedy death; had complained somewhat of dyspeptic symptoms accompanied with obstinate constipation; but these symptoms received but little notice. While in this condition was seized with profuse hæmatemesis, which was arrested, but left him so prostrated that he sank into collapse and died in about 36 hours thereafter." The *post-mortem* was made within 24 hours after death. In the posterior wall of the stomach was a large opening, of the size of a 25 cent piece. Its edges were sharply cut. Behind the ulcer the anterior wall of the pancreas presented, the edges of the opening being very slightly adherent to that viscus—the adhesion strong enough, however, to prevent the effusion of the contents of the stomach.

PERFORATION OF APPENDIX VERMIFORMIS.

By Dr. Pilcher: "R. M., æt. 54 years; in robust health. After an attack of diarrhœa, lasting four days, attacked with pain, not very intense, in bowels, for which a mild opiate was given; after the lapse of three hours pain became agonizing; its maximum intensity being referred to the right iliac region, whence it radiated over whole abdomen. Pain con-

tinued uncontrollable; tympanitis became marked, and death ensued 66 hours after first symptom of pain.

Upon autopsy the whole peritoneal surface was found injected; buttery lymph covered the serous surface of the intestines, gluing them together; no serous or sero-purulent effusion present. A perforation by ulceration in the *appendix vermiformis*, just within its orifice, as shown in the specimen presented, was found. No foreign body was discovered either impacted in the appendix or loose in the peritoneal cavity. The *gall* bladder was much dilated, and contained over 200 calculi." The autopsy was made 37 hours after death.

Dr. Read was reminded of a case in which he made an autopsy when an interne in the Long Island College Hospital, some years ago. There were seven perforations of the small intestine. The man was brought in by the police, without any history, except that he had been found in a condition of intoxication; but it was subsequently ascertained that up to that time he had been apparently healthy. He died in two days after admission to the hospital. No inflammatory action could be made out. In reply to Dr. Jewett, he said they could scarcely have occurred *post-mortem*, as the autopsy was made within 24 hours after death.

Dr. John Merritt : Some time last spring a portion of the small intestine with a perforation was presented for me, and, as I was unable to come myself, no history accompanied it. It was from a man who had quarreled and been kicked in the abdomen. The autopsy was made within a few hours after death. It was not a laceration, but an ulceration.

HEMORRHAGIC INFARCTIONS OF THE LUNGS.

Dr. Segur presented portions of a lung showing hemorrhagic infarctions. It was removed from the body of a man who was admitted to St. Peter's Hospital in a dying condition. There was general anasarca.

Post-mortem.—The serous cavities all contained large amounts of fluid. There was valvular disease of the *heart* and chronic contraction of the *kidneys*. The *lungs* were congested and slightly œdematous and contained numerous dark masses of coagulated blood, some of large size. In the kidneys were several cicatrices supposed to be the remains of former infarctions.

HEMORRHAGIC INFLAMMATION OF THE GENITO-URINARY PASSAGES.

Dr. Westbrook : The specimens were removed from the body of a man about 70 years old, who died in the Long Island College Hospital. There was a history of painful micturition extending over a few months prior to his admission to the hospital. When admitted, in the service of Dr. L. D. Mason, he was suffering from retention of urine. Dr. Beasley, the House Surgeon, passed a catheter and drew off about 50 oz.

of urine. The latter portion of it was stained a deep red with blood. During the few days of his life in the hospital the catheter was repeatedly used, large quantities of urine, always bloody, being withdrawn. He soon died of exhaustion.

At the autopsy the body was extremely emaciated, but showed no evidence of disease except in the urinary organs and a little nodular thickening of both pulmonary pleuræ at the apices. The kidneys were contracted and hard. The mucus membrane of their pelves and infundibulæ was thickened and dark red with dilated arborescent vascularity. The ureters were similarly affected. The bladder projected as high as the middle of the symphysis pubis and was about the size of a base ball. Its color externally was dark blue or purple, as if it contained clotted blood. It contained a little bloody urine. The mucous membrane was extremely thick and corrugated (partly due to contraction), dark red and spongy, and bearing everywhere upon its surface a yellowish deposit, which was apparently a layer of mucus and phosphatic matter. The urethra was in the same condition and had the same layer of deposit upon it, looking something like a false membrane. In the membranous portion, apparently where the catheter had caught in this layer of deposit, the latter was torn up, simulating a laceration of the mucosa itself. In the inferior portion of the *prostate* a fibrous tumor as large as a filbert was found. This had undoubtedly narrowed the urethra.

Dr. Rockwell: Was there any history of former hæmaturia? It is known that the sudden evacuation of large quantities of urine is sometimes followed by hemorrhage. Dr. Jewett asked if the urine was all drawn off.

Dr. Westbrook: The urine was all drawn off. There was a history of previous hæmaturia. He would like to ask Dr. Rockwell in which direction he thought the inflammation was propagated in this case.

Dr. Rockwell judged from the comparison of the appearances in the different parts that the trouble had begun in the bladder and extended upward through the ureters of the pelves.

PARASITES OF THE MEATUS.

Dr. J. S. Prout showed the microscopical appearances of a vegetative growth which he had found in the external auditory meatus. He was unable to classify it, as it was unlike anything that had previously come under his observation.

PUERPERAL PHLEBITIS.

Dr. Stephenson, of St. Peter's Hospital, presented the pelvic organs from a woman dead of puerperal fever. The doctor went, at the request of the sisters of the hospital, to visit the patient, whom he found upon the floor

of a garret in the most wretched condition. Two weeks before she had been confined at the seventh month, but, owing to insufficiency of care and nourishment, had remained ill. On his first visit he found her in a burning fever, the temperature 104° F., with diarrhoea, pains in the abdomen, a weak and rapid pulse, and great prostration.

She was removed to the hospital and treated with stimulants, opium, and sponging of the body with luke-warm water and vinegar. The abdomen was distended, the thighs somewhat flexed, but owing to the low delirium it could not be determined whether she had any pain or not. On the day following her admission the fever increased; the tongue became dry and brown; the delirium was more marked; distention of the abdomen greater; flexion of the thighs increased.

The temperature rose to 106° F., the pulse was very rapid and feeble and the respirations irregular and gasping. The attending physician, Dr. B. F. Westbrook, diagnosed peritonitis but was unable to determine the cause of it, inasmuch as the vaginal discharge was slight and inoffensive, and neither on vaginal examination or external manipulation could anything but tympanitis be discovered. In the hopes of relieving the thoracic organs Dr. W. inserted a medium-sized operating needle into the intestine on the left side. Considerable gas escaped and the distention was lessened. Cold water was applied to the abdomen, and Quin. Sulph. \mathfrak{ij} . administered per orem. Stimulants were freely used. She died within a few hours.

Autopsy.—Head not examined. The *thoracic viscera* presented no notable lesions. The intestines were inflated and the peritoneum dry and injected. No trace of the perforation of the needle could be found in the intestine, though the puncture in the abdominal wall was easily identified. No gas had escaped from the intestine into the peritoneal cavity. The abdominal organs were normal except the *kidneys*, which presented the appearance of acute nephritis. On the left side of the brim of the pelvis there was a tumor about two inches in diameter, which lay upon the psoas muscle. It proved to be the left ovary with its venous plexus and the corresponding portion of the broad ligament and fallopian tube. The peritoneum and connective tissue were thickened and the veins of the ovarian plexus were inflamed and contained pus. The lower 3 to 4 inches of the ovarian vein was also thickened and contained pus. The upper portion of the vein was collapsed.

The uterus presented the appearance common at that time. At the fundus, however, and more upon the left side, a portion of the wall had the dark discoloration of inflamed tissue. No pus was found in the uterine veins. No pyæmic abscesses.


Dr. Westbrook had not heard of the *post-mortem* investigation having

been made in previous cases of tapping the intestine, with a view to ascertaining the effect of the puncture upon the gut.

Dr. Segur: There have been two or three cases reported in this Society, some years ago, in which such investigation was made, but the puncture could not be found.

Dr. Rockwell spoke of a case in which the late Dr. Giberson had made several punctures. They were not to be found *post-mortem*.

—The Curator would be pleased to receive correspondence in regard to the exchange of microscopical slides. Address, Dr. E. S. BUNKER, No. 280 Henry Street, Brooklyn.

 *The Secretary requests members presenting specimens to present therewith a written account of the history and pathological appearances. An observance of this request on the part of the members would insure a much more satisfactory report of their cases.*

BENJ. F. WESTBROOK, *Secretary*.

THE MEDICAL SOCIETY OF THE COUNTY OF KINGS.

ABSTRACT OF THE MINUTES OF THE REGULAR STATED MEETING, FEBRUARY 17TH, 1880.

The President, DR. C. JEWETT, in the chair. About 80 members present.

The minutes of the annual meeting, and of the adjourned annual meeting, were read, and, after slight amendments, were approved.

There were no applications for membership.

The Council reported the election to membership of Jesse B. Lung, M.D., Coll. P. & S., Keokuk, 1877; Charles E. De la Vergne, M.D., L. I. C. H., 1878.

DR. J. H. STERLING read a paper on "Naso-pharyngeal Catarrh."

The paper was discussed by Drs. Burge, Sherwell, Bennett, Mathewson, Squibb and Walker.

DR. J. J. LAMADRID read a paper entitled, "A Case of Craniotomy, with Remarks."

DR. J. H. H. BURGE read a paper entitled "A New Obstetric Forceps." This paper was discussed by Drs. Otterson, Wight and Sanford.

The paper of Dr. Alexander Hutchins, entitled, "Forced Alimentation," was read by title.

A paper entitled "Trismus Neonatorum," by Dr. John Cooper, was read by the Secretary, Dr. Wyckoff.

DR. A. HUTCHINS, referring to the action of the Society, at the adjourned Annual Meeting, in voting him one hundred dollars from the funds of the Society, to be expended in books for his private library, in recognition of his services in connection with THE PROCEEDINGS, acknowledged the courtesy of the Society, but begged the privilege of diverting the gift by making it a personal contribution to the library of the Society. On motion of DR. SQUIBB, seconded by DR. BURGE, the Society granted the request.

On motion, Drs. J. C. Hutchison, J. S. Wight and J. H. H. Burge were appointed a committee to consider the practical suggestions in a paper read by Dr. Prout, in relation to the management of hospitals, and report at the next meeting.

DR. W. WALLACE, referring to a paper read before this Society some months ago by Dr. Merritt, upon "Ready Method of Testing Pepsin," inquired whether it had been given up or merely postponed.

DR. HUTCHINS replied that by direction of the Council it had been indefinitely postponed.

The matter was discussed by Drs. Gray, Hutchins, Squibb, Kretschmar, Wight, Sanford, Westbrook, Walker, Hawley and Merritt, and on motion of Dr. Gray it was

Resolved, That a committee of three be appointed by the President to inquire into the legal aspects of this matter, and report at the next meeting of the Society.

THE CHAIR appointed Drs. Gray, Sanford and Wyckoff as such committee, and the Society adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρωποῖσι, κακῶν θελυτῆρ' ὀδυναῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—THE COMMITTEE ON BUILDING are ready to receive subscriptions to the Building Fund. It is hoped that the members will bear in mind that a strong Society is of material help to all its members, and nothing will add more to the strength of the Society than to have our own building. So let every one aid with a will.

Subscriptions can be sent to any member of the Committee, viz.: E. R. Squibb, C. R. McClellan, B. A. Segur, J. D. Rushmore, Geo. G. Hopkins.

—MEDICAL SOCIETY OF THE STATE OF NEW YORK.—At the late meeting the Medical Society of the County of Kings was represented by Drs. Byrne, Chapman, Hopkins, Hutchins, Hutchison, Rockwell, Rushmore, Shaw, Sherwell, Squibb, Wyckoff. Dr. Wyckoff was elected a permanent member.

—THE COMMITTEE ON LIBRARY, ordered at the Annual Meeting, are now engaged in canvassing the city for funds wherewith to purchase valuable books of reference for the Library. The response, so far as known, has been so liberal that a handsome addition to the Library may be anticipated. It is probable that a full list of contributors will be published in the April number of THE PROCEEDINGS.

—THE ANNUAL DUES for 1880 are FIVE DOLLARS. The Treasurer would be gratified at an early recognition of this fact.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The March meeting will be held on the 16th, at which there will be presented the following papers:

Radical Treatment of Alveolar Abscess, by Dr. J. N. Farrar.

Abuse of the Abdominal Section, by Dr. T. M. Rochester.

A Case of Paralysis of all Extremities, due to a Myelitis of the Anterior Cornua, by Dr. L. C. Gray.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

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PROCEEDINGS
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RADICAL TREATMENT OF ALVEOLAR ABSCESS.

BY J. N. FARRAR, M.D., D.D.S.

A human tooth is composed of four principal tissues: 1st. The pulp, or internal portion. 2d. The dentine, a tissue covering the pulp, made up of tubes more or less calcified, containing substances having functions analogous to those of nerves, and which radiate from the pulp through it in vast numbers. 3d. The cementum, clothing the outside of the root portion of the dentine, and which resembles true bone in its anatomical structure, and is principally nourished by its socket periosteum. 4th. The enamel covering the crown portion of the dentine and which is nourished from the pulp through the dentine.

As the nourishment of the enamel and dentine depends upon the pulp, and the cementum is principally nourished by the peridental membrane lining the socket, it will be seen that a pulpless tooth is not necessarily entirely dead; and, although the enamel and dentine of a pulpless tooth is lifeless, the cementum may continue to live and preserve the usefulness of the organ. This point should be borne in mind throughout the rest of this paper, as these are the conditions of a so-called abscessed tooth. Alveolar abscess is a disease outside of a tooth, and located in some portion of the socket, generally at the bottom.

Acute alveolar abscesses, in process of formation, is generally, though not always, accompanied by pain and swelling in the region of the dis-

eased part, and, as a result, loss of sleep and exhaustion, indicating that there should be speedy relief by local depletion or by the letting out of pent-up pus (if it be present), to avoid greater systemic exhaustion and increased destruction of tissue.

Of course the shortest route to a cure and the most radical treatment of the disease lies in the extraction of the associated tooth; but, unless it is worthless, or the condition and circumstances of the patient contra-indicate it, such treatment would be far from skillful. It would be like that of amputation of the finger to get rid of whitlow. The grand object of dentistry is to save teeth, not to destroy them; and I trust that the necessity of saving useful teeth for assisting in the maintenance of health, to say nothing of their esthetics, is too well understood by every person present to need further argument on this point.

It is not my purpose to dwell upon the "let alone plan," or to speak much upon the poultice persuasion, of endurance of days of agony and sleepless nights followed by terrific destruction of tissue, for it would hardly come within my idea of rational radical method of treatment. Should it, however, be adopted, care should be exercised to avoid external opening on the face. If poulticing be resorted to it should always be applied to the gum and not the outside of the face, but either plan is not only unwise, but a long way out of the difficulty.

Such diseases may be the result of blows upon, or perhaps of the so-called Riggs disease, a disease which leads to detachment of the periosteum from the tooth, but oftener and almost universally these abscesses arise from death of the pulp. There may be, however, causes such as lead to abscesses in other parts of the body, which are sometimes unassignable. Such cases are so rare, however, that a life-time of practice may not see one. Such alveolar abscesses are generally located on the sides of roots of teeth, without necessarily involving the pulp. As hinted before, socket abscesses generally arise from decomposition of the nerve pulp or other foreign substance pent-up in the pulp-canal, or perhaps from the decomposition of the internal or possibly that of the apical portions of the root.

The decomposition of the pulp causes evolution of gas, requiring several times its former space to easily accommodate it, and not finding sufficient vent, seeks escape through the foramen, accompanied possibly by portions of other elements of the decayed pulp, which poisons the tissues around about the end of the root, leading to congested thickening of the inter-osseal membrane and its immediate alveolar surroundings, forming a sort of tumor, which will generally, unless the gas be allowed to escape, increase, and ultimately break down by decomposition in its central portion, forming pus, evolving more gas, followed by increase of diseased

territory and destruction of alveolar tissue, leading to a condition not identical with, but analogous to that which exists in whitlow, with gas between it and its ossific attachments, which is increased in quantity by the addition of exudation, which forces it more and more from the cementum, causing, at the same time, destruction of the cancellated alveolars (socket) behind and around the sac, until, generally, if not opened early by artificial means, increases in size, and generally finding vent some-

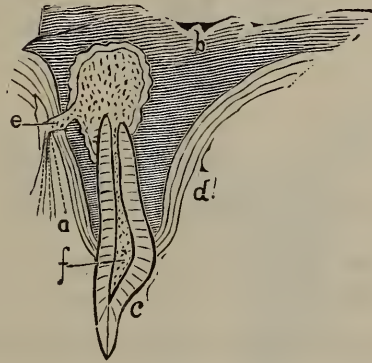


FIG. 1.—Section showing an Abscess on a Superior Central Incisor Tooth.

where by nature's own process. The pus, in such cases, extends along the line of least resistance until it breaks through one of the plates of the process, generally in the immediate vicinity ; but it may extend along between these tables to a considerable distance before final escape. Sometimes this opening occurs in the posterior part of the mouth, or into and perhaps through the cheek, chin, and possibly it may burrow along between the muscles, and finally open under the jaw or in the neck, generally on the same side, but sometimes on the opposite side, causing intense pain, sometimes jeopardizing life. They may also open into the antral chamber, and may or may not discharge through the nasal cavity. This latter variety, with its treatment, forms an interesting phase of the subject; perhaps, however, more especially to me, since I have materially improved instruments for the treatment, but want of time will prevent me from its further consideration.

There is now a cavity in the alveolar process, lined with thickened and diseased periosteal tissue, with inflammatory deposits forming a cyst. The sac is lined with what the old writers called the pyogenic membrane, but which, in fact, is a pathological surface, within which there is pus detritus, and into which is more or less projected the end of the dead portion of the root.

As hinted, the basis structure of the walls of the sac generally has its origin more or less in periosteal tissue, which goes on increasing in thickness by the ordinary process of excitation and inflammation and by the

interposition of exudation, ultimately becoming more or less organized within and among this fibrous tissue.



FIG. 2.—Outlines of an Abscess on a Molar Tooth.

It has been said that the sac is primarily formed from the periosteum forced from the root. While this is true in many cases, it is not always simply the lifting of the periosteum bodily from the cementum, for sometimes a considerable portion of the membrane remains upon the root, while only shreds of it are traceable in the sac.



FIG. 3.—External view of an abscess in the author's collections.

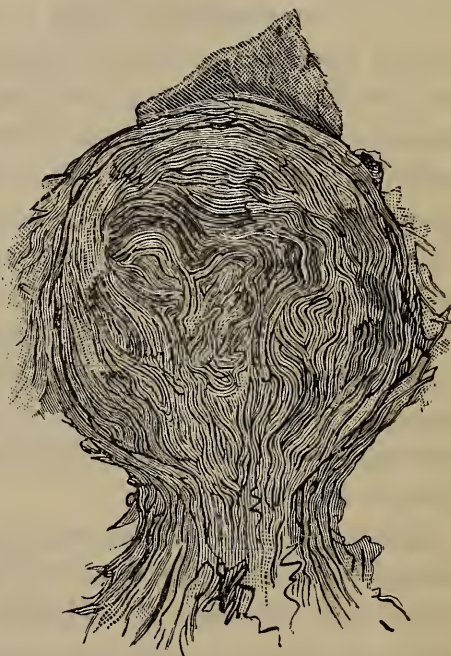


FIG. 4.—Section of the same abscess cut through the centre, showing the appearance of one of slow growth.

This disease, acting through the agency of its heated pus detritus, tending to cause decomposition of the dead portion of the root, also, some-

times, through some chemical vital process in, about and along the territorial line, between the living and the dead, hastens by this cementitis its own destruction, showing its works in the formation of irregular pockets into the cementum, sometimes penetrating even through it into the dentine, leading sometimes to total destruction of the corresponding extremity of the root, leaving it so ragged as to render a permanent cure impossible unless the root first be rounded off by some artificial means.

That which takes place in the cementum, along the quasi vital line, by the same cause is also carried on to a more extensive degree in the adjacent alveolar process.

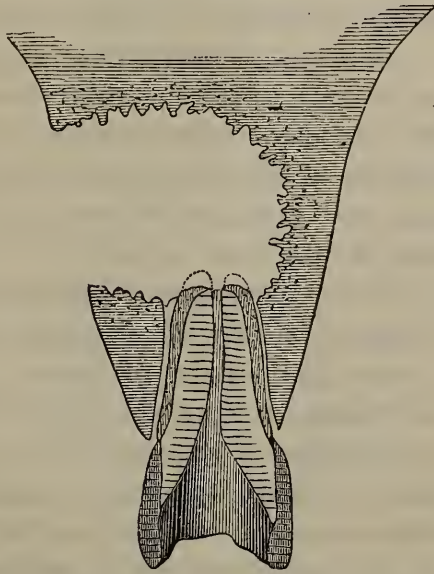


FIG. 5.—Section of tooth and alveolar process, showing the results of an alveolar abscess in an upper bicuspid tooth. From a specimen in author's collection.

Alveolar abscess, therefore, may be found in all stages of development, thus calling for considerable discretion in management. This depending upon the duration of the disease, age and health of the patient, condition and quality of the tooth, periosteum, socket, and especially the stage or degree of degeneracy of the dead portion of the root, which in such cases is like so much foreign matter, and is liable to act like it. But perhaps this may not be true of some very recent cases, which, although I am not positive, I believe, like cases of whitlow, when properly treated early, may possibly, in some instances, be followed by a reunion of the periosteum with its former ossific associate. If this be true the importance of early and radical treatment of both diseases must be apparent to all.

If the apex of a root be dead, this portion of the tooth under such circumstances, of course, must be considered somewhat in the light of a foreign substance, and notwithstanding in most cases, by disinfecting the

root, the disease may be permanently cured. Others are impossible, and even if cured the dead apices of such roots are liable, sooner or later, even after the most skillful management, to undergo further degeneration, causing re-formation of another abscess in the place of the one formerly cured ; especially is this liable during attacks of sickness and failure in the tone of the general health, as in the case of an old soldier's encysted bullet, which during some decline in health may cause the re-opening of an old wound that had been healed a long time.

It was only last week I was called to the bedside of a young lady supposed to be dying from measles, when, after she had rallied a little, the opening of an alveolar abscess, which had formed while in this condition, enabled her to immediately and rapidly improve. Much might be said upon the importance of timely and radical treatment of such dental troubles during periods of other diseases, for, as must be evident to all, the timely treatment of alveolar abscess, like that of lancing gums of children, will sometimes save life by removing the weight, which, added to other diseases, would result in death.

In the treatment of these abscesses our object should be to arrest the decomposition of living tissue and the evolution of gas, in order to prevent pain and greater destruction of tissue. Any drug, therefore, which will arrest this degenerative process, and at the same time stimulate the adjacent living tissues to a healthier tone, is desirable. Of all medicines now known for simple abscesses, especially chronic cases (unaccompanied with necrosis), it seems to me that creosote is equal to any, if not the best, because it has all of the desired antiseptic and stimulating virtues in a high degree. But although this is my favorite drug in ordinary cases, there are those of larger size, especially those accompanied with necrotic conditions, which would improve better under acid preparations ; such for instance as chloride of zinc, aromatic sulphuric acid, etc. In my opinion, contrary to some authorities, we should not aim to destroy the sac, but to restore it to its former condition and position as nearly as possible. Our treatment should have in view, so to speak, the establishment of a sort of "line of demarkation" in the cystic tissues, by the stimulation of all the cells or tissue elements of the sac and surroundings that are capable of restoration to a higher degree of vitality, and the getting rid of those that are worthless. We should also endeavor to dislodge stagnant blood, and re-establish freer circulation, thereby stimulating nutrition of the part. There are two methods of application of medicine : First, through the pulp canal of the tooth ; second, by treating it directly through the gum and alveolar process. There is still another form of treatment, followed by a few operators, which is to pass through the alveolar process a burr

drill of considerable size and cut away all the diseased territory, sac and bone, on the ground that there is always a necrotic condition of the process, a condition which in fact is not often present.

This method of treatment is old and heroic enough for anybody, but it does not appear to me rational unless there is actual necrosis, and even then it is doubtful if it be the best plan.

Having made the diagnosis our first object should be to devise the best means of reaching the seat of the disease. To explain my views upon this is my present object.

Rational, radical treatment of disease, as I understand it, consists in the use of the right medicine in the right place at the right time, administered or applied in the right way and in the most direct manner.

When located about the front teeth, especially if there be a fistulous opening, the abscess may generally be easily reached and successfully treated by the old method of pumping the medicine through the pulp canal, with a swab on a small instrument; but abscesses located about the roots of bicuspid and molar teeth, although they may be treated, and sometimes successfully treated too, by the uncertain method of root dressing, cannot as a rule be radically treated, so that chance success shall be almost if not quite certain, except the medicine be applied more directly and through the gum by means of some instrument of the nature of a syringe. One that can be operated with safety and with comfort to the patient is preferable. Such an instrument I show you here. Until I devised this there were none practically painless, because the working of the piston saturated with creosote was so difficult that it could not be manipulated with sufficient care.

If it be difficult to treat abscesses already having fistulas, how much more so it must be to get medicine into them through the pulp canal when there is no fistula; almost impossible, unless the foramen be first drilled larger than found in nature, which is about the size of a horse hair, and even if made larger it would be like trying to pour liquid into a bottle already filled, and besides this, it would be extremely difficult to properly drain the diseased part without leaving the tooth open, which would be liable to soon fill with food, and above all render the pulp canal difficult to fill after a cure, without getting some of the filling pushed through the foramen, into the seat of the abscess, thus causing more trouble and loss of the tooth, as is often the case after this method.

After a drainage through the alveolar process has been formed, either by nature or by some artificial means, the preparatory step in the treatment of this disease is to remove the cause of the abscess by thoroughly cleansing and disinfecting the interior of the pulpless tooth, after which it may be temporarily closed to prevent ingress of food, but more espe-

cially to compel any discharge to escape through the fistula, thus assisting in keeping it open until the disease shall heal from the bottom first.

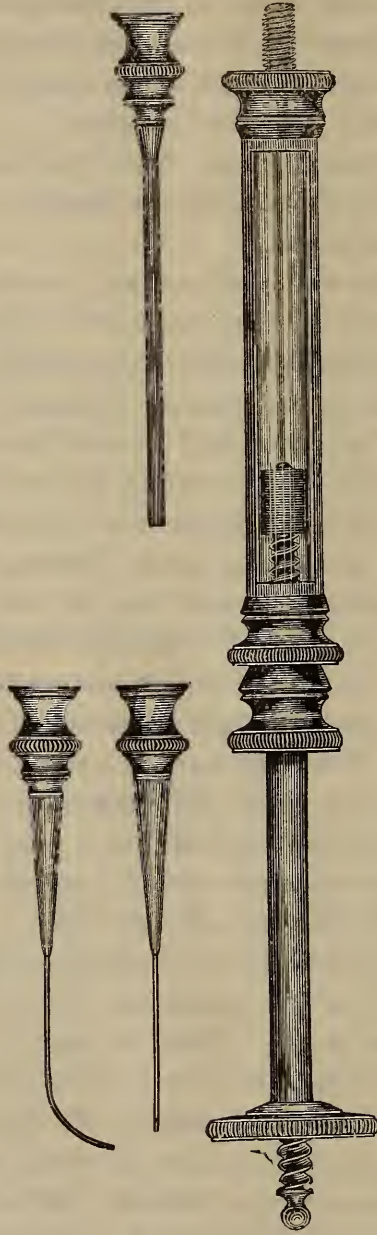


FIG. 6.—Farrar's Alveolar Abscess Syringe.

Should the route of the fistula be tortuous it is generally best to make a new and direct passage through the gum to the opening in the alveolus.

This accomplished, the treatment is at once made simple, and generally effectual, by the use of a syringe. To those who may urge that such treatment may be too painful I will say, that in this business, bold yet careful operators, who have a thorough knowledge of the anatomy of the parts, are generally more successful than timid people; and even if

considerable pain be caused at the first operation (which is not often necessary) there is seldom much in subsequent ones if done properly.

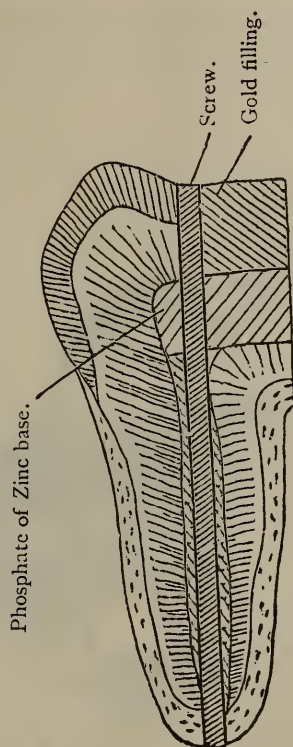


FIG. 7.—Section of a Half-Molar showing how a large Foramen may be safely filled with a Gold Screw.

Much might be said upon the various routes through the gum and alveolus to the seat of the abscesses on the various teeth; but to an audience of this intelligence this seems unnecessary. But those who wish to un-

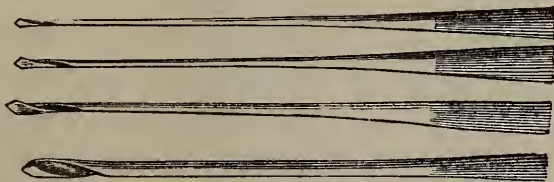


FIG. 8.—Drills for making Artificial Fistulas through the Alveolar Process.

derstand my customary methods can refer to these diagrams, illustrating more particularly the more heroic methods of operations for artificial drainages. In this case you will also see the instruments used in my daily practice.

Incompatibility in the use of drugs should of course be avoided. For instance, creosote, not readily uniting with water, if mixed with previous water injections might not be as beneficial as desired. I have known globules of wood creosote, under such circumstances, to gravitate and act

only on the lower portion of a large abscess. While all this is true, however, there need be no difficulty if it first be mixed with alcohol, which readily unites with it.

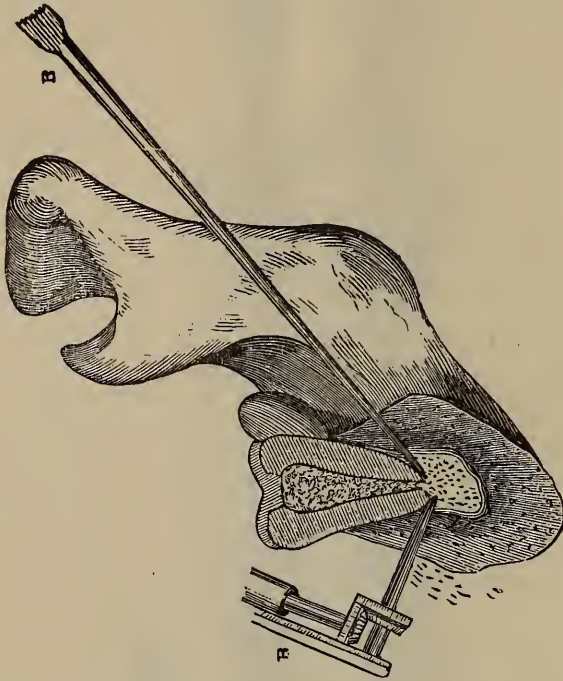


FIG. 9.—Section of Lower Jaw, showing Abscess on a Second Molar Tooth, and the method of producing an Artificial Fistula.

Any strength of dilution of creosote may in this way be made which will suit any condition of simple abscess. While chronic abscesses seem to call for more potent strengths, more recent ones often act more favorably under the weaker ones.

If the syringe be used with care the operation, after the first or second, will seldom cause pain. This method does not necessarily imply much time; brevity is one of the great advantages of the syringe method. To a person not accustomed to it it is surprising to know how brief are most operations. Excepting the first operation, which often requires five or ten minutes or more, most injections can be made in less than a minute.

In looking over the records of time used in sixteen successive treatments, I find they average only $16\frac{1}{4}$ seconds each. The longest time being 32 seconds, the shortest only 3 seconds.

As a rule one injection is not sufficient to effect a cure. Allowing sufficient time and opportunity for nature to act, it should be repeated once in three or five days until cured. Although the common practice of plugging the foramen at the apex of the pulp canal with a small amount of creosoted cotton may be practicable, when free from associa-

tion with abscesses (or even after the abscess is cured), I think there is less danger of future trouble from absorption of impurities if the extreme apical portion of the canal be plugged with gold, and the balance of the canal filled with gutta-percha to cut off sudden thermal changes.

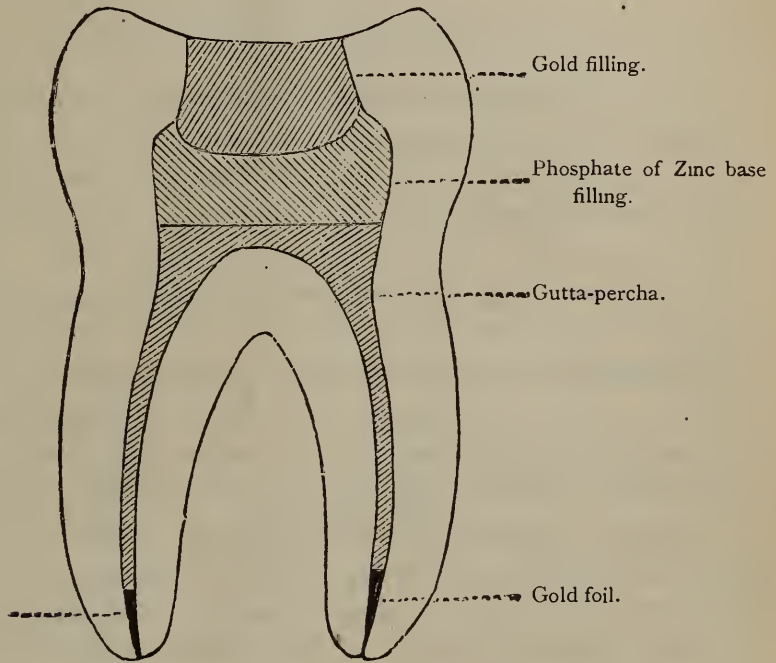


FIG. 10.—Section of Molar Tooth showing how an ordinary abscessed root should be filled.

DISCUSSION.

DR. J. H. HUNT had made a microscopical examination of the alveolar abscess alluded to; and what particularly struck him was the concentric formation of the layers of fibrous tissue which made up the walls of the sac. It appeared like the walls of an old aneurism with fibrine running around the edges; or like the layers of an onion—the centre being filled up with broken-down detritus or débris. That peculiar formation was very interesting.

THE ABUSE OF THE ABDOMINAL SECTION.

BY T. M. ROCHESTER, M.D.

A short time since the writer was present at a discussion on the matter of resort to the abdominal section, in the course of which the following statements were made: It was said that with the present antiseptic system that the abdominal section had ceased to be a formidable operation, and its much more frequent use was strongly urged. It was stated that we should resort to it in tubal and abdominal pregnancies so soon as the diagnosis was made. Some even went so far as to say that it was not only justifiable, but even eminently proper that the abdomen should be opened, when, for example, an internal strangulation of the intestines was *suspected*; saying that if such trouble did exist you might thereby save the life of a patient; and, on the other hand, if it were a simple case of obstinate constipation, which had failed to yield to the ordinary remedies, the wound could be closed and no harm done, while you would then have the satisfaction of being able to form an accurate diagnosis.

And, furthermore, when the writer felt compelled to make an earnest protest to each of the above opinions, one gentleman went so far as to say that having made the abdominal section for the relief of a strangulation of the intestines, a surgeon would fall far short of his duty should he not free the gut, even if it were found necessary to divide the abdominal aorta in order to accomplish this end; that this was a necessity to be deplored of course, but was the only thing to be done under the circumstance. Possibly the gentleman who made this last statement was carried away by the heat of the discussion, and did not realize what he was saying; and this I believe to be the most charitable construction to put upon his words.

Much more was said in harmony with the above opinion, but I think that I have recalled enough to excuse me for appearing before the Society with a paper on a subject so important as the abdominal section. And the writer hopes that his paper will be the means of promoting a discussion on this topic, so that if he has been in the wrong 'in his protest he may learn it; but if on the contrary in the right, that he may be sustained by the authoritative approval of those in the profession who are qualified to speak from a practical experience in the matter. And with this end in view it will not be necessary to weary you with long quotations from authorities, or almost endless statistics *pro* and *con*.

It is not proposed to discuss in detail the vast subject of ovariectomy, gastrotomy, laparotomy and the various other abdominal sections, but merely to enter a protest against the performance of any of these sections without due consideration, or simply for the *éclat* of an operation.

For the sake of clearness let me formulate the position I took in opposition to the above-mentioned opinion:

1. That the abdominal section, for whatever cause performed, is one of the gravest surgical procedures, even with the present antiseptic method.
2. That, in case of acute intestinal obstruction, the abdomen should not be opened unless there is great certainty as regards the diagnosis, and not then as a rule, when it is a case of intussusception.
3. That it should not be opened in tubal pregnancy until after rupture of the sac.
4. That the male abdomen is very much less tolerant of surgical interference than the female.
5. That under no circumstances whatever should the abdomen be opened for diagnostic purposes.

I claim to be as progressive as any one, but "*festina lente*" is a good maxim for a surgeon to remember, and we should always use great judgment in our progress, as with us it is human lives that are at stake.

Let us look at ovariectomy. Here we have a condition particularly favorable for an operation. The presence of the tumor has inured the peritoneum to handling, as it were, and we find that delicate membrane thickened and toughened by a chronic inflammation. And yet in these cases, where the greatest care is taken, where the operation is performed by the most eminent men in our profession, the death-rate is over thirty per cent. In my own experience with those that I have witnessed the mortality has been over sixty per cent. Of course, there are operators in whose hands the death-rate has been reduced to a minimum, notably among these Keith of Edinburgh, who has performed seventy consecutive operations without a single death. But in making up the statistics we must take the average results of all operators, and doing this we find the mortality as high as above stated.

How with any sort of fairness can this be called a simple operation? It is a grand one to be sure, and has been a great boon to suffering womankind, and I do not wish for a moment to be understood as protesting against it. I have only said these few words to show that opening the abdomen, even for this cause, is a grave procedure. How much more so then, when, as in a case of acute intestinal obstruction, we have not those favorable conditions. Here the attack is sudden, coming on within a few days, and the peritoneum, teased by the existing troubles, is ready to burst into a flame of inflammation. Again, much more has to be done

than in ovariectomy. The bowels must be turned over to find the seat of injury, and when found suppose them to be black, sphacelated, ready to drop to pieces; will you have aided your patient by an ocular inspection of the trouble? Or suppose you remove the sphacelated portion, stitch the edges to the external wound and recovery ensues. Is your patient not left with that most disgusting of all things, an artificial anus, from which death itself is a happy relief? During the previously mentioned discussion I was *reminded* that it was not customary to turn over the bowels with "chain-hooks and harrows," and that to a very great extent the risk of subsequent inflammation was avoided by operating antiseptically. But it is necessary to lift up the contents of the abdomen, and to disturb them from their place, in order to find the seat of the trouble. And this is attended with great risk, no matter how carefully it be done; and furthermore, I do not believe it possible to even cut the healthy peritoneum with the sharpest scalpel without bruising it. Surely it is as sensitive as a flower-stalk, which is not unbruised when divided with the keenest edge.

Antiseptics, at the most, only prevent the lodgment of bacteria and other germs. They do not prevent the damage done by the necessary handling of the contents of the abdomen. Then, too, the experiments of Stimson, as recorded in the American Journal of Medical Sciences for January, 1880, seem to prove that carbolic acid is not in reality the germicide that it has been thought to be, and that the possible advantage of its surgical employment is due as much, if not more, to the necessarily increased cleanliness of hands, instruments, sponges, and bandages from the use of the spray and different solutions, as it is to the actual germicidal properties of the acid. Dr. Gross, of Philadelphia, simply enforces the rule of perfect cleanliness in all the accessories for an operation, and reports as good results as those who employ the most rigorous antiseptic precautions.

Again, I am not quite sure but that harm is done by the use of the spray itself. Evaporation is so rapid that the bowels and surrounding tissues are in consequence greatly chilled, and eminent ovariectomists are divided in their opinion, and are searching for something to take the place of carbolic acid which shall be less irritating in its action. Then, too, one of the most frequent causes of death after these operations is shock. Do antiseptics ward off this ill consequence? Are they not rather promotive of it from their chilling effect?

As regards intestinal obstruction from other causes than the impaction of foreign bodies, Bryant says: "There is no class of cases which calls for more thought and judgment; for the question of operative relief has to be decided on very questionable grounds, and it is yet an open question as to whether the procedure be admissible." But he adds that this

does not apply so forcibly to those cases where the obstruction is caused by the binding down of the gut by a band of adhesion, and suggests that these present more favorable conditions for operating. Let me give you, very briefly, the notes of a case of this kind, however, which came under the observation of a friend of mine: Carl X., male, aged 32, admitted to Roosevelt Hospital, February 20th, 1876. Three weeks previously had received a blow on the abdomen in jumping from a moving train. Six days prior to admittance the usual symptoms of intestinal obstruction came on. No benefit from medical treatment. At 4:30 P. M. the abdomen was opened for his relief by Dr. Sands, assisted by Drs. Weir, Markoe and Jacobi. The bowels were held in place by cloths wrung out of warm water. The large intestine was found greatly distended, and strangulated by a band of adhesion between the mesentery and the vertebral column. The strangulation being relieved by dividing this band, a profuse evacuation of the bowels followed. The pulse was good throughout, except just toward the close of the operation, and then responded readily to stimulants. By 7 P. M. the bowels had moved freely several times, but at 10:15 P. M. the patient died of shock in spite of all that could be done for him.

Of gastrotomy proper, except for the removal of a foreign body, both Ashurst and Bryant say that it is an operation of expediency only, and a very doubtful expediency at that. The former records eleven cases, of which ten died, and he had not learned of the result in the remaining one. As regards intussusception most authorities unite in considering it bad surgery to operate for its relief. Even if reduction is impossible by the usual methods of inflation, posture, tapping and others, the bowel may spontaneously free itself. Or if sphacelation has occurred it not infrequently happens that an adhesive inflammation is set up, and a cure effected by the sloughing, and discharge through the rectum, of the invaginated portion. Now with these two possibilities to hope for, after the failure of treatment, it seems a rash procedure to operate except in rare and exceptional cases; and this is especially so, since the chances of being able to accomplish our intended object are very uncertain, even after the abdominal section has been made.

Before leaving this branch of the subject it would be well to revert to the relative safety of an operation on the male or the female abdomen. It is a surgical fact that the female abdomen is much more tolerant of operative measures, especially in the case of a woman who has borne children, than the male; and this is in a great measure due to the occasional and physiological presence of the gravid uterus, and the consequent pushing up and crowding back of the intestines and other organs, thus rendering them more susceptible of handling than those of the

male. This fact should be an important factor in confirming or opposing our decision, in those cases of strangulation or intussusception of the bowels, when the indications seem to point towards an operation as the last resort.

In conclusion let us glance for a moment at the question of an early surgical interference with tubal and abdominal pregnancies. I believe that the best authorities will bear me out in the statement that it is not proper, as a rule, to operate until after rupture of the sac ; and when this accident happens it seems to me that elytrotomy would be preferable to the abdominal section, especially when the rupture occurs at an early period of gestation. Cohnstein, of Heidelberg, uses this method in all cases demanding surgical interference.

At a meeting of the New York Obstetrical Society, in 1878, Dr. Noeggerath quite severely criticised Dr. Thomas for operating in a case of abdominal pregnancy of seventeen months' duration ; and he stated that he had known the foetus to remain in the abdomen for twelve and thirteen years and longer, without the slightest danger. He further said that he spoke from a very extended literary experience, and that all authorities united in saying that abdominal tumors of every description should not be meddled with, unless they threatened the functions of some important organ or the life itself of the patient.

But of all abdominal tumors, I believe that tubal pregnancies should especially be labeled, "Hands off," as far as the abdominal section for their relief is concerned, at least until after rupture of the sac has occurred. In 1879 Dr. Paul F. Munde stated before the New York Obstetrical Society that in future he should attempt to deliver all cases of tubal or interstitial pregnancy *per vias naturales* ; either by dilating the uterine extremity of the fallopian tube, or by scraping an opening through the sac. Dr. John L. Graham reported two cases, at between the fourth and fifth month of gestation, so delivered spontaneously. In the first of these, after consultation with Dr. W. L. Atlee, they had decided to operate by abdominal section on the succeeding day. Fortunately, however, nature took matters into her own hands, and the foetus was delivered through the uterus and vagina that night. Learning from this he wisely left the second case alone with a like happy result. Dr. H. Lenox Hodge also reported two cases in his own practice, when, assisted by his father, Dr. Hugh L. Hodge, he effected delivery, either by dilating the fallopian tube or scraping through the sac. One of the cases was at eight and a half months, and the child was born alive, and I believe still lives. Barnes likewise recommended this mode of procedure.

In the face of such eminent authorities, and especially in the face of such good results from this less heroic treatment, shall we still continue

to recommend the abdominal section in these cases, and expose our patients to the very great risk of death from peritonitis, shock or the other causes of a fatal issue? Nature is a kinder and a better physician than any of us, and he who follows her indications most closely will achieve the best results. Let us assist her all we can, but not run counter. Boldness and promptitude are prime virtues in a surgeon, but he can have no worse faults than recklessness and a lack of good judgment. A surgeon in these days is one who must not only know how to use his hands, but his head also. Is it necessary to say anything about the utter impropriety of severing the abdominal aorta for the relief of a strangulated gut? or to condemn the opening of the abdomen for a colic, which may *possibly* be something else? I think not. Surely if it is bad surgery to probe an abdominal wound, it is worse surgery to make one for the discovery of an injury that may not exist.

Brief and imperfect as my remarks have been, if what I have said this evening shall prevent a single abdomen from being opened unnecessarily, or without due and careful consideration, I shall feel more than satisfied.

THE PLACE OF ABDOMINAL SECTION IN SURGERY.

BY N. B. SIZER, M.D.

During the last few years many of our surgeons are beginning to realize that science and art have not been usually allowed to furnish *all* their aid to those deplorable cases in which a strangulated viscus, an impacted gall-stone or a bleeding point within the peritoneum threatens speedy death. A very suggestive case appears in *Hays Journal* (Oct., 1878, p. 321), where Atlee, of Philadelphia, narrates a successful case of laparotomy, to remove a foetus which came to full term four months before the operation.

Dr. Harris, who appends valuable remarks, considers that when rupture of the foetal envelopes threatens life the chief obstacle to successful interference is the difficulty of early diagnosis, and that, in those cases where gestation goes on to full term, the patient's life is ever menaced; rupture of the sac, peritonitis or septicæmia being likely to occur unexpectedly.

At a recent meeting of the Royal Medical and Chirurgical Society, of London (*Lancet*, Nov. 15th, 1879), Mr. Lawson Tait reported a case of extra-uterine foetation at the third month, which he had mistaken for

hematocele, and had operated exploratory. The incision allowed the foetal débris to escape, and the patient recovered.

Mr. Tait remarked that he made it a rule to open the abdomen whenever there seemed a possibility of doing good, and in nearly sixty cases he had never regretted his action.

Mr. Doran related a case, where, after a second attack of hematocele, Mr. Spencer Wells had advised gastrotomy, but was overruled by the relatives; six weeks later a third hemorrhage suddenly ended the patient's life. Mr. Doran said that in view of the great mortality in these cases he was in favor of antiseptic exploration. He had noticed that, since the introduction of the Listerian procedure, the peritoneum seemed to be clean in fatal cases, and he therefore believed that those deaths were not of septic origin.

Mr. Knowsley Thornton narrated a similar case, and said that no doubt the proper course was early interference.

Mr. Spencer Wells remarked that he had lately seen another fatal case, where gastrotomy had been tendered, but refused by the friends.

Since he had adopted antiseptic precautions he had done 81 operations with 6 deaths ($7\frac{1}{2}\%$), whereas in the series immediately preceding the mortality had been nearly 10%. He considered antiseptics a great gain, and was much more confident now in urging an operation upon the patient and her friends.

Dr. Matthews Duncan remarked that he had seen three cases of extra-uterine gestation since coming to London, but he had not cared to interfere. The patients were all alive yet.

Mr. Tait thought Dr. Duncan's cases were in great peril, although every foetus were dead. He had found that spontaneous recovery by discharge of the foetal débris was very rare; he had seen only one case to 10 or 11 fatal ones. He advocated immediate operation, as soon as the diagnosis was made.

At the May meeting, 1878, of this Society, the late Dr. Giberson presented the history of a most interesting case, the patient dying one month after the first symptoms, of a fifth hemorrhage in a tubal pregnancy, the diagnosis being reasonably certain from the very first. Dr. G. justly remarked that a second (certainly a third) hemorrhage would justify the suspicion of a misplaced pregnancy. May we not add "and justify an immediate operation"?

In the February number of the "Annals" of the Anatomical and Surgical Society there is presented an intensely interesting series of cases considered from this point of view. Dr. Williams' case of intestinal obstruction is of much value, as showing that at *no time during life* would anything but abdominal section have been useful. But the patient expired before any operation could be done.

Mr. Jessop, of Leeds (*Brit. Med. Jour.*, Sept. 27th, 1879), strongly advises operation in these cases when the diagnosis is well made.

Teale says emphatically that the chances of recovery are by no means lessened by an exploratory incision, if the case is found to be irremediable after opening the abdomen.

He maintains that antiseptic exploration will exclude any dread of peritonitis, and that all the arguments that were used in the past against ovariectomy are equally valuable and forcible against modern antiseptic laparotomy, and yet ovariectomy is a recognized procedure.

As to the cause of intestinal obstruction, Mr. Gay, Dr. Hilton Fagge, Dr. Brinton and others have shown that it is *purely mechanical* in a large percentage of cases, and therefore remediable by operation.

Dr. Fowler's case ("Annals," p. 67) of rupture of the pampiniform plexus is entirely analogous to those of rupture of the sac in the early weeks of misplaced pregnancy, and the report of the autopsy is singularly suggestive when it states that no source of hemorrhage was visible except the ruptured vein. How easy would ligation of the bleeding point have been had it been *outside* of the abdomen instead of *inside*!

The four cases of tubal pregnancy immediately following Dr. Fowler's case are very interesting, but cannot now be mentioned more largely. The details are sufficiently sad, not only to excite our sympathies, but to stimulate our best efforts to do, if possible, more than stand idly by and watch death seize his victim.

We believe that as no case of rupture of the sac in the early months has been known to recover, the duty of the surgeon seems imperative in this class of cases.

If no interference is had the patient *must* die. Life may be saved by a capital operation, from which, by the aid of antiseptics, a very large percentage recover. To my mind the argument in this class of cases is all on one side.

As to cases of misplaced pregnancy going on to full term, the recoveries must be very rare, the patient succumbing to exhaustion from hectic or septicæmia.

During the discussion following Dr. Read's paper ("Proceedings," Jan., 1880) it was remarked that, when pregnancy is abdominal, antiseptic laparotomy, so successfully done by Barnes and Thomas, will probably be established as the operation of the future. When the cyst lies in contiguity with the vagina, elytrorophy through the cul-de-sac by the thermo-cautère of Paquelin may be preferred. In conclusion let me say that no surgical question of to-day is fraught with more vital interest than that which decides the propriety of abdominal section in cases like those we have already briefly reviewed.

We believe that the expectant theory will fail us here, where in many cases the patient *must* die without an operation, and *may* die with it.

Certainly the mind of a conscientious surgeon will be more at rest when his patient has died after he has done all he could bring to his aid, rather than when he sees him expiring on the expectant plan, the surgeon meanwhile sitting by in inglorious ease, waiting for that favorite goddess of the sluggard, the "*vis medicatrix naturæ*," to step in and help him.

DISCUSSION.

DR. SANFORD had been especially interested in the subject of abdominal section, for the reason that an operation had been performed in his own family for the removal of an ovarian tumor more than two years since, and four of his neighbors had also been subjected to the same operation during the last four years, all of whom recovered.

The case in his own family referred to, as stated before, was successful; but there was one point in connection with it which had only recently come to his notice. The operation was performed by Prof. Thomas, of New York, and, in every sense, was successful. Everything went right until about three months after the operation. The lady, fifty years of age, was about as usual. The speaker at that time being away—out of town—a neighboring physician was sent for, she having complained of what she said was a partial rupture of the abdominal wall. The physician, on examination, said he could not discover anything; he thought it was probably her imagination. A few months afterwards, she still complaining of the trouble, he examined her again, and thought he then discovered that there was a rupture, or an attempt at a partial rupture, of the abdominal walls below the umbilicus, and pretty close down to the pubis. Taking his word for it, he (Dr. S.) never examined her until recently—being urged to do so by reason of her frequent and increasing complaints, for the relief of which she had put on a binder for support—when he was very much surprised to find the most complete and extensive rupture ever examined or seen by him. The entire abdominal wall, commencing at a point just above the pubis, of a triangular form, which apparently spread to either side, to the upper extent of the original wound, so that the space in the abdominal walls was sufficiently large to be covered by his hand! or nearly 2 by 3 inches. Through this rupture the intestines were constantly protruding. She had been taken to a prominent instrument-maker for the purpose of having her fitted with a truss, and, although a cure has not been effected, very much had been accomplished for her relief. The ingenuity of the instrument-maker will no doubt be able to devise some form of support; but as yet all forms of abdominal supporters fail to keep up the protrusion for a moment. The intestine protrusion is almost as large as one's head! It seems as if almost all the intestines would come out.

She has been a sufferer during the last year from colic. It seems to be connected with this rupture—a sort of partial strangulation, so to express it.

He had never found any one who had seen anything of this kind as the result of a carefully performed operation of this sort.

The question in regard to modifying influences of operations in midwifery seems to be brought out, or to be touched upon, in the papers read; especially in reference to the fact that surgical operations are better borne by women than men. This may be

one reason why opening the abdominal walls is not more generally fatal in women. Opening into the uterus, in cases of pregnancy, is a decidedly more serious operation, and, therefore, in his opinion, they should not be classed among the same line of cases. He was aware that statistics seem to indicate that Cæsarean section is an operation which should be resorted to in very rare cases.

DR. F. W. ROCKWELL, in answer to a question by Dr. Sanford, said that he had the pleasure, recently, of hearing a paper by Wieber on operations for peri-typhritis. He had operated four times. He makes a very long external opening; but he stated that in these four cases two of them were troubled with hernia after the operation. Both were relieved after a few months by the use of an ordinary truss.

DR. BURGE was decidedly in sympathy with the views of Dr. Rochester, as expressed in his paper. He believed in the more conservative plan in such cases. Sometimes the conservative surgeon is pointed at as a timid surgeon; but he was willing to be called "timid" if timidity means being afraid to sacrifice human life!

DR. HUTCHISON did not think any surgeon can be governed in any case by statistics given by other surgeons. Every case must be judged of by itself.

He was in entire sympathy with the views expressed by Dr. Rochester in reference to conservatism in these operations. There are cases when conservatism may not be admissible.

Within two weeks he has had occasion to perform laparotomy twice.

One of these cases he saw on Saturday (6th inst.), a German woman in the Eastern District of the city, who received some injury on Sunday previous. There was some sort of a scrimmage in the house, and by some method, which he did not find out, she received an injury of the abdomen, which, on the following Tuesday, was followed by severe abdominal pains. She was seen by Dr. O. H. Smith, of the Eastern District, who supposed it was a simple colic, and treated her accordingly. After two or three days stercoraceous vomiting took place. The doctor reported to him that she had had no fever, that on the morning of the 6th the temperature was 96.05° , but that the thermometer had not remained sufficiently long to secure an accurate register. The pulse was 90 per minute; there was some tenderness on pressure of the abdomen, and some tympanites, especially in the right iliac form extending upward. The poor woman within the previous 24 hours had failed, and she had had the usual treatment for acute obstruction of the bowels—opium, injections of water and air—except the introduction of a needle into the bowels, to let off the gas, which was not indicated, as the tympanites was slight. It was evident that, unless the patient was relieved by a surgical operation, she would die. She was considered a proper case for opening of the abdomen. An incision was made three inches in length between the umbilicus and pubis, and the intestines showed themselves at once, highly congested and covered with lymph. He introduced his fingers into the abdominal cavity for the purpose of ascertaining whether there was an obstruction. The presence of peritonitis was a surprise, as the symptoms did not indicate that condition. In passing the fingers over to the right iliac fossa a large quantity of pus gushed out. It had no fœcal odor. When the fingers were subsequently carried around to the left, a large quantity of pus was discharged from that point also.

Now when the abdomen was opened there were no indications of pus; but on pulling up the intestines on either side, over the iliac fossa, large quantities of pus were discharged. The quantity was estimated at a quart, but that was rather over the true measure; but there certainly was over a pint. The pus was all taken out and the abdomen washed out thoroughly with sponges soaked in a solution of carbolic acid, and the wound was closed. The patient was then left with the expectation that she would

die. She suffered a good deal of a shock from the operation, but she had no further vomiting. It was necessary to give opium afterwards, for the purpose of relieving pain and make her comfortable. At the end of thirty-six hours she died. No post-mortem could be obtained, and of course the exact nature of the obstruction could not be determined. He imagined, however, that in this case there were fibrous bands which obstructed the intestines. They were probably in the ileum; if they had been in the jejunum there would not have been stercoraceous vomiting, and there were no symptoms of obstruction in the colon, such as tenesmus, bloody discharges from the rectum, in addition to stercoraceous vomiting, etc. Therefore a diagnosis was made of obstruction somewhere in the ileum. He supposed that the inflammation had been local on each side. The whole case is unsatisfactory, because of no *post-mortem*; but that the operation should have been done was very evident to him, and that the obstruction was released was evident from the cessation of vomiting. He would not have been satisfied that he had done his duty to the patient if he had not opened the abdominal cavity.

On the following Thursday he opened the abdominal cavity for the removal of a tumor, which was diagnosed a fibroid tumor, hanging from the top of the uterus. It was about as large as one's head, and occupied a central position. It could be moved readily by the hand from side to side, was solid, and a sound could be introduced into the uterus five inches. The examination satisfied him that it was a fibroid tumor growing from the top of the uterus, and attached to the uterus by a narrow pedicle.

He made an incision in this case from the umbilicus to the pubes, and subsequently extended it three inches above the umbilicus, being careful as he proceeded to arrest the hemorrhage. He thought that if the introduction of the sponge or other foreign substance could be avoided it would be desirable to do so. He was particularly careful to arrest the hemorrhage, and thus protracted the operation considerably. After the tumor was pushed out through the abdominal walls, two veins were found running over it to the omentum, which were tied with antiseptic ligatures. The ligatures in that case, and they are such as he generally uses, were made antiseptic by combining two drachms of salicylic acid and one drachm of carbolic acid, with a sufficient amount of yellow wax to make an ounce. Thus the antiseptic wax contains $12\frac{1}{2}\%$ of carbolic acid and 25% of salicylic acid. Ligatures soaked in the mixture while hot are made antiseptic. This mixture was prepared for the doctor by Heydenreich Bros., and first suggested to him by Dr. Skene. After these veins were cut a needle with an eye in the point was passed from before backwards through the pedicle, so as to have a double ligature. Immediately the pedicle was punctured a copious hemorrhage took place, of a venous character. He did not understand why this should be, but supposed that a large venous-sinus had been punctured. The object was to cut off the tumor where it joined the uterus, but it was difficult to determine the exact line of demarkation. After tying the two veins and the pedicle, the hemorrhage was still quite copious from the puncture. He then surrounded the whole pedicle with a ligature, and arrested the hemorrhage quite promptly. The pedicle was then divided and the tumor removed.

A good deal of blood, notwithstanding the efforts to prevent it, got into the abdomen from the wound, and it was necessary to sponge it out with new sponges, thoroughly clean and soaked in carbolic acid, one part in about twenty, until the sponges showed scarcely a stain. Mr. Keith lays a great stress upon this point, *i. e.*, getting out every particle of blood, although Mr. Lister holds that by the use of carbolic acid the danger from this is reduced to a low degree. Keith, however, insists upon the removal of blood.

After the pedicle had been dropped back into the abdomen he was so anxious that there should be no hemorrhage—for he believed it to be a case for good results—that he drew up the pedicle afterward, there being a suspicion of oozing from it, and applied the thermo-cautery, searing the whole end of the stump; then he thought he was entirely safe. Then it was dropped back into the abdomen, a drainage tube properly applied, and the wound closed.

This operation was done on Thursday. Within twenty-four hours she began to manifest some symptoms of failure, and the gentleman who had her in charge suspected hemorrhage. Indeed, hemorrhage manifested itself somewhat in the drainage tube. This hemorrhage went on until Sunday night, when she died.

Upon *post-mortem* a considerable amount of blood was found in the abdominal cavity, and the cause of death was hemorrhage from the stump.

It should have been remarked before that this was a syphilitic patient, and he supposed that the disposition to hemorrhage was due to an aplastic condition of the blood, or a condition something like that which exists in a purpuric patient.

Both of these operations were done under the spray, strictly according to Lister's method, although he really did not have much faith in it.

Dr. Z. T. EMERY remarked, referring to Spencer Wells' use of the spray, that he believed that the more recent experience was decidedly against it, the danger being too great to render it of very great benefit.

Dr. L. S. PILCHER expressed the opinion that it is no longer necessary to discuss the propriety of the abdominal section, or to enter into any discussion in regard to the dangers, inherent or otherwise, which are likely to arise from the operation. The question for discussion is in what cases is it proper to use abdominal sections?

Dr. Hutchison had very properly remarked that every case must be decided on its own merits, for by no statistics is it possible to form a judgment by which an opinion can be based, as to the propriety of performing an operation in a particular case. He has detailed two interesting cases which prove the assertion.

As to the abuse of the operation, he was inclined to think that there was far more danger of abuse in not performing an operation than in rushing into it hastily. The dangers resulting from this operation, we are beginning to find, are less grave than was formerly supposed.

Dr. G. K. Smith had, during the previous evening, assisted Dr. Kissam in the performance of an operation for strangulated hernia. It was a case of considerable interest. The patient was 84 years of age, and in a very bad condition at the time of the operation; and there was some discussion as to the propriety of the operation, as to whether it had better be performed. There was some discussion also about the condition of the heart; there was a mitral regurgitant murmur. There came to his mind the saying of the small boy in the country on a certain occasion: "When you don't know anything for certain, you can come pretty near to it by guessing." So they guessed and performed the operation—or, rather, Dr. Kissam did. He performed it well, indeed, but in an hour the patient was dead! So it happens that when we are hunting up statistics and trying to find just what is the best thing to do, there is still a great deal which we cannot decide by their assistance. This, however, he would say: There are some surgeons who love surgery so well that they cannot let the brain govern the hand in the use of the knife. There are some surgeons who can see their duty in the use of the knife, but who cannot see their duty in withholding the knife from their patients.

Some years ago, it may be twenty or so, Dr. Mitchell, now in Hornellsville, sent for Prof. Frank H. Hamilton for the purpose of performing ovariectomy. He went to the

house of the patient prepared for the operation; but before the operation was decided on, the patient asked him: "Doctor, what are my chances of life for five years to come, or for a few years, should the operation not be performed? I have daughters," said she, "who are growing up, and they need a mother now more than they will need me afterwards; they will need a mother in future as they did not in early childhood, and if I can be spared a few years longer it will be a great service to them." Hamilton took that question into consideration, and decided not to operate, and the *vis medicatrix naturæ*, so much maligned, took care of her, and she lived to a good old age. The tumor disappeared of its own accord.

He had seen surgeons who were so anxious to operate, that if it were his own individual case he should be loth to engage them. He had seen them so desirous of operating that one would think their main object was to be reported in the journals as having performed a great surgical operation.

With regard to statistics, most men are sure to report their successful cases, and many men are just as sure to report those which are not successful.

THE MEDICAL SOCIETY OF THE COUNTY OF KINGS.

ABSTRACT OF THE MINUTES OF THE REGULAR STATED MEETING, MARCH 16TH, 1880.

The President, Dr. Charles Jewett, in the chair.

The minutes of the previous meeting were read by the Secretary and approved.

The Chair announced that, since the last meeting of the Society, Dr. J. H. H. Burge had been made Chairman of the Committee to consider certain points in the paper of Dr. Prout.

The following gentlemen were proposed for membership:

Newly proposed: R. T. Davidson, 13 Troutman St., E. D.; Dr. C. H. Johnson, 315 Degraw St.; Dr. H. A. Fairbairn, 239 McDonough St.; Dr. P. H. Prendergrast, 518 Henry St.; Dr. S. J. Sorensen, 132 President St.

The Secretary announced the acceptance by the Council of the following, as members of the Society:

Newly elected: A. M. Burns, M.D., Bellevue H. M. Coll., 1871; J. P. Oliver, M.D., Univ. Pa., 1861; J. M. Raub, M.D., Univ. Pa., 1874.

Dr. J. N. Farrar read a paper entitled "The Radical Treatment of Alveolar Abscess," which was briefly debated by Dr. J. H. Hunt.

Dr. Thomas M. Rochester read a paper entitled "The Abuse of the Abdominal Section."

The Chair stated that the reader of the next paper announced on the card, Dr. Gray, had expected to exhibit a patient in illustration of the subject, but for some reason the patient had disappointed him. The paper would, therefore, be postponed until the next meeting of the Society.

After the next paper a discussion would be in order upon it and that read by Dr. Rochester.

Further along in the evening Dr. Hughes explained the absence of Dr. Gray: He was prevented from attending by reason of a very sick child, who was dangerously ill.

Dr. N. B. Sizer read a paper entitled "The Place of Abdominal Section in Surgery."

The two papers were then discussed by Drs. Sanford, Rockwell, Burge, Hutchinson, Emery, Pilcher and G. K. Smith.

The Committee on Dr. Prout's paper, through Dr. Burge, Chairman, reported progress, and announced that the complete report would be presented at next meeting.

The Committee on the Legal Liabilities and Responsibilities of Publishers, through Dr. Sanford, reported progress. He had no doubt but that the Committee would be able to make a final report at the next meeting of the Society, which would include a written opinion from a member of the legal profession. Report accepted.

Dr. E. R. Squibb said that he had a paper bearing upon the subject in hand. It was entitled "The Relations of the Medical Profession to the Business Aspects of the Materia Medica."

A short discussion ensued as to the advisability of reading the paper at this meeting; but it was decided to postpone the paper until the next meeting, and have it read in connection with the report—due announcement being made on the cards.

Dr. Read, in behalf of the Committee to solicit subscriptions for the library, reported the great success of the Committee, inasmuch as the sum of five hundred dollars, nearly, had been secured, and yet all the members of the profession of Brooklyn had not been called upon. The Committee therefore reported progress, which report was accepted.

The Chair announced the intention of the Society to hold a Social Meeting, in commemoration of the success of the efforts to increase the library, at some future time, and the Society adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρώποισι, κακῶν θελκτῆρ' οδυναῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—A NUMBER of complaints have been made of failure to receive the March number of THE PROCEEDINGS. The conductors have no comprehension of the cause of the non-delivery. If any who failed to receive a copy of the March number will notify the office of THE PROCEEDINGS, the deficiency will be made good.

—THE REPORT OF THE COMMITTEE of the American Pharmaceutical Association, on Revision of the U. S. Pharm. in this year, is just published for public discussion and criticism in the profession. This Report is a closely printed book of 200 pages, for gratuitous distribution, and it bears witness to an expenditure of valuable time and labor, which can hardly be appreciated by those unfamiliar with such work. Its successful accomplishment is wholly due to the Chairman of the Committee, Mr. Charles Rice, of Bellevue Hospital, and it deserves to be known in the profession that the labor was voluntarily contributed from the overtime of active duties well performed, and that the work was prosecuted to its successful termination through broken health.

Of the merits of the work done, and of the utility of it to the approaching Convention, little need be said. But the character of it and its very timely publication should certainly stir up the medical profession to a little interest in its own most important work, as involved in the Convention of May next. And it may have some influence in arousing the torpid medical organizations to send good delegations to the Convention, instead of leaving everything in the hands of the pharmacists.

—DR. RICHARD NORRIS, an English physiologist, recently read a paper before the Birmingham Philosophical Society, on a new corpuscular element in the blood.

He claims that normal mammalian blood contains a considerable proportion of colorless bodies, identical in all respects with the red blood corpuscle, except in the absence of color.

These bodies are invisible in the liquor sanguinis because they have the same refractive index as that fluid. To render them visible, Prof. Norris either withdraws the liquor sanguinis, or changes its density by the addition of a saturated solution of common salt. He bases upon his observations a theory that these colorless discs are a forming stage of the red blood corpuscles in which they have not yet acquired hæmoglobin, and that they have their origin in lymph corpuscles, which have assumed a bi-concave form on being introduced into the blood.

The London *Medical Record*, of Jan. 15th, 1880, contains a critical note upon the researches of Dr. Norris, by Mrs. Ernest Hart. On repeating his experiments she observes the same appearances, but thinks they are artificially produced by the method of pumping the flow.

She believes the colorless corpuscles of Dr. N. are mainly red corpuscles from which the hæmoglobin has been discharged by the pressure of the cover-glass, or by the well-known action of the salt solution.

The colorless discs, which undoubtedly do exist naturally in blood, she holds to be not an earlier, but a later stage of red corpuscle, while undergoing a post-mortem change, prior to taking part in the formation of fibrine. On this point Mrs. H. promises to publish further observations.

—THE PATHOLOGICAL SOCIETY'S REPORT could not be prepared for this number, by reason of the severe illness of the Secretary.

—IN THE GLASGOW SANITARY JOURNAL for March, 1880, occurs a paper by Dr. Carmichael, whose object was to determine by experiment whether *specific* germs could pass through a well-constructed water-trap.

—HISTORY OF MEDICINE IN NEW JERSEY AND OF ITS MEDICAL MEN, from the settlement of the province to A. D. 1800, by Stephen Wickes, A.M., M.D., pp. 449. So far as the searching of records through dim and remote periods, this history is a monument of patient and effective labor. It is a compendium of medical ancestry, gathered from innumerable resources, and grouped with literary taste and skill worthy of its distinguished author. One can hardly fail, in turning its elegant pages, to feel a warmth and tenderness toward the memory of the fathers who preceded us, in times more troublous, when the doctor was so often the soldier, fighting for his country and applying himself to all the tender duties belonging to his art. The author has done an excellent work. It would be well if his work would stimulate others in other States to put in permanent form the history of the actors in the early days of our profession.

—THE QUARTERLY JOURNAL OF INEBRIETY, published at Hartford, Conn., is the only journal published devoted to the medical study of inebriety, alcoholism, and opium mania. There is no branch of medical

or social science of more interest and less understood by all classes. It comes home to every one who is interested in this topic, full of suggestion and instruction, pointing out the practical solution of one of the most difficult problems of modern times. This journal occupies a field entirely new, and is the organ of the American Association for the Cure of Inebriates.

—BOOKS WANTED.—Any one having a copy of Zeimmsen's Cyclopædia, Niemeyer's Practice, Holmes' System of Surgery and Trousseau's Clinical Medicine, for sale at a low figure, may hear of a purchaser by addressing a note to Capt. Chartres at the Rooms.

—DR. G. R. CUTTER's Dictionary of German Terms Used in Medicine has been reviewed by nearly every medical journal in the land, and thus far scarcely one word of adverse criticism has been uttered. We congratulate Dr. C. that his laborious undertaking has come so near perfection.

—IODOFORM PASTE.—The *Medical Gazette* advises the following combination for the purpose of disguising the odor of the drug: R. Iodoformi ℥i; Mucil. cum glycerina gtt xx; Ol. Menth. pip. (seu neroli, seu caryoph.) gtt i; M.

—"REST, position and pressure are the trinity of the healing surgical graces, but the greatest of all is pressure."—Sampson Gamgee in *Am. Practitioner*, February.

—DR. JAMES J. LARKIN died at 190 Leonard Street, E. D., March 10, in the forty-third year of his age. He was a native of New York City, and a graduate in medicine from the University of that city in 1859. He had resided in Brooklyn since that time. His fatal disease was hypertrophy of the heart with hydrothorax.

—THE INDEX MEDICUS.—This highly important journal is about entering its second year. We bespeak for it a generous support among studious and professional physicians.

—A LOVING CUP—everybody knows what that is—has been presented to the New York Academy of Medicine by a rich lady who is an admirer of our profession. Could we not have three or four more of those cups distributed, with advantage, in this latitude? Also, what kind of fluid is appropriate to this kind of cup?

—RADWAY'S READY RELIEF is said to be composed of soap liniment 8 parts, aqua ammoniæ and tincture of capsicum, each one part.

The following verbatim copy of An Act now pending in the Legislature is published for the information of the profession:

STATE OF NEW YORK.

No. 444.

IN ASSEMBLY,

February 16, 1880.

Introduced by Mr. HUSTED—read twice and referred to the committee on the judiciary—reported favorably from said committee and committed to the committee of the whole.

AN ACT

Authorizing physicians of the different medical schools to meet in consultation.

The People of the State of New York, represented in Senate and Assembly, do enact as follows :

1 SECTION 1. It shall be and is lawful for the physicians and sur-
2 geons of any medical school duly recognized under the laws of this
3 state to meet in consultation, in cases of sickness, with physicians of
4 any different system of practice now recognized by the laws of this state.

1 § 2. No county, state or local medical society shall in any way or
2 manner discipline or punish any member of such county, state or local
3 medical society for any aid, assistance or counsel he shall render to
4 any physician of a different school of practice.

1 § 3. All acts in conflict with this act are hereby repealed.

1 § 4. This act shall take effect immediately.

—ANIMAL VACCINE.—Dr. De Pietra Santa recently addressed the Paris Academy of Medicine a cogent argument in favor of animal vaccination, a practice which the French profession have been slow to adopt. He does not deny the efficacy of humanized vaccine, but claims that the bovine variety has certain marked advantages over the former.

—THE SANITARY ENGINEER, London, is a monthly journal and register of sanitary appliances, which much resembles, and will largely take the place of the late *Public Health*. Vol. I. of the new series began January 6. The price is 7s. Address C. L. Marsh & Co., 138 Fleet Street, London, E. C.

—THE FOLLOWING SUBSCRIPTIONS TO THE PROCEEDINGS HAVE BEEN RECEIVED IN RESPONSE TO A CIRCULAR ISSUED IN FEBRUARY: It is hoped that the publication of this list may refresh the minds of many who have, perhaps, forgotten the circular, and who may be anxious to do their share in forwarding the interests of THE PROCEEDINGS.

Drs. S. G. Armor,	Drs. E. Hasbrouck,	Drs. T. M. Rochester,
G. H. Atkinson,	G. G. Hopkins,	F. W. Rockwell,
S. B. Childs,	J. C. Hutchison,	J. D. Rushmore,
A. H. Clark,	T. M. Ingraham,	W. F. Sanford,
J. P. Colgan,	J. A. Jenkins,	S. Santoiro,
F. W. Colton,	C. Jewett,	P. L. Schenck,
— Conroy,	J. J. Kunz,	F. A. Schlitz,
J. Cooper,	J. R. Leaming,	J. C. Shaw,
T. P. Corbally,	— Love,	A. W. Shepard,
— Cronyn,	H. Lowenstein,	A. J. C. Skene,
De La Vergne,	W. Maddren,	E. R. Squibb,
A. J. Dower,	A. Mathewson,	J. D. Sullivan,
G. R. Fowler,	E. F. Mordough,	C. H. de Szigethy
H. J. Garrigues,	G. P. Oliver,	E. A. Whaley,
W. Gilfillan,	S. H. Olmstead,	H. B. White,
L. C. Gray,	W. C. Otterson,	L. A. Willis,
W. E. Griffiths,	W. H. Peer,	K. C. Woodhull,
S. C. Griggs,	H. N. Read,	F. W. Wunderlich.

CONTRIBUTORS TO LIBRARY FUND.

On the opposite page will be found the list of subscribers to the Library Fund. The handsome result of the efforts of the Committee will be accepted with pleasure by the members of the Society. Other subscriptions are promised, and it is possible all the members have not been visited. The Committee are now at work in the selection of such Medical Works as will prove of permanent value and most general use.

Drs. Ambrose, D.	Drs. Griffiths, W. E.	Drs. Palmer, E. W.
Anderson, Wm.	Griggs, S. C.	Prendergast, P. J.
Armor, S. G.	Harcourt, J. M.	Pendleton, E.
Ayres, B.	Harrigan, J.	Pilcher, L. S.
Baker, G. W.	Harvey, E. J.	Prout, J. S.
Ball, Jno.	Hawley, J. S.	Randolph, W. H.
Barber, J. H.	Henderson, A. C.	Rappold, J. C.
Bliss, W. A.	Hillyer, J. H.	Richardson, J. E.
Bodkin, D. G.	Hopkins, Geo.	Read, H. N.
Bowron, F. W.	Hunt, J. H.	Robinson, S. C.
Brady, S. J.	Hutchins, Alex.	Rochester, T. M.
Briggs, B. M.	Hutchison, J. C.	Rockwell, F. W.
Brush, G. W.	Hyde, J. W.	Rooney, A.
Buell, R. M.	Jewett, Chas.	Russell, W. G.
Bunker, E. S.	Joye, T.	Schenck, P. L.
Burge, J. H. H.	King, J. S.	Schmetzer, G.
Byrne, Jno.	Kissam, D. E.	Schmidt, C. F.
Catlin, A. W.	Krauter, J.	Segur, B. A.
Chase, W. B.	Kretzschmar, P.	Shepard, A. W.
Chapman, E. N.	Kuhn, G. R.	Sherwell, S.
Childs, S. B.	Lamadrid, J. J.	Silberman, Wm.
Church, S.	Leary, J. B.	Sims, H. C.
Clark, C. F.	Lindridge, E. F.	Skene, A. J. C.
Clarke, Alex. S.	Little, W. A.	Small, H. G.
Colgan, J. P.	Loewenstein, H.	Smith, E. P.
Corbally, T. P.	Maddren, Wm.	Snively, J. C.
Corbin, Job.	Malone, Edw.	Squibb, E. R.
Corcoran, J. J.	Martin, W. H.	Sterling, J. H.
Crutchley, W. T.	Matheson, A. Ross.	Stuart, F. H.
Curry, O. M.	Mathewson, A.	Stub, A.
De Bowes, T. N.	Matson, N.	Sullivan, J. D.
Dower, A. J.	Matson, W. B.	Sweeney, Jas.
Drake, N. S.	McClellan, C. R.	Szigethy, C. A. H.
Drury, Geo.	McCorkle, J. A.	Thayer, W. H.
Eddy, Chas.	McCosker, T.	Vanderveer, J. R.
Edson, Benj.	McLean, H. C.	Van Ness, J.
Ensell, J. E.	Moore, J. F.	Wade, J. E.
Eskens, F.	Mordough, E. F.	Walker, J.
Evans, G. A.	Morissy, W. P.	Wallace, W.
Farrar, J. N.	Morse, V.	Watt, Jas.
Fearn, H.	Mulligan, E. L.	Welton, R. B.
Figueira, M.	Murphy, D.	Welty, G. W.
Ford, A. W.	Newton, B.	West, F. E.
Ford, N.	Nightingale, L. B.	Wieber, Geo.
Fowler, G. R.	Olcott, Chas.	Williams, H. F.
Frankum, T. H.	Olcott, C.	Williams, W. H.
Freeman, J. N.	Olmstead, S. H.	Willis, L. A.
French, T. R.	Ormiston, Robt.	Wilson, J.
Frickenstein, T.	Ostrander, G. A.	Woodhull, K. C.
Fuller, S. E.	Otterson, Andrew.	Wunderlich, F. W.
Gleavy, J. J.	Otterson, W. C.	Wyckoff, R. M.
Goodwin, F.	Paine, A. R.	Zellhoefer, C.
Gregory, J. E.		

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The April meeting will be held on the 20th, at which there will be presented the following papers:

A case of Paralysis of all Extremities due to a Myelitis of the Anterior Cornua, by Dr. L. C. Gray.

Notes on Pepsin, by Dr. P. H. Kretzschmar.

The Relations of the Medical Profession to the Trade Interests of the Materia Medica, by Dr. E. R. Squibb.

Recent Views on Fracture of Inferior Extremity of Radius, by Dr. L. S. Pilcher.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President</i>	C. JEWETT, M.D., 310 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
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J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK.

(1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI, Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

DELEGATES TO THE AMERICAN MEDICAL ASSOCIATION.

MEETS IN N. Y. CITY JUNE 1st, 1880.

Dr. Andrews.	Dr. Fowler.	Dr. A. Otterson.	Dr. Skene.
" Armor.	" Griffiths.	" Pilcher.	" Vanderveer.
" Baker.	" Hopkins.	" Reese.	" Wallace.
" Barber.	" Hutchins.	" Rockwell.	" B. F. Westbrook.
" Bartlett.	" Hunt.	" Rushmore.	" Wight.
" Bodkin.	" Mason.	" Sanford.	" Wyckoff.
" Byrne.	" Mattison.	" Shaw.	" Wunderlich.
" Catlin	" Mitchell.	" Sherwell.	

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Drs. R. M. Buell.
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

MAY, 1880.

No. 3.

A CASE OF PARALYSIS OF THE FOUR EXTREMITIES AND THE MUSCLES OF THE TRUNK, DUE TO MYELITIS OF THE ANTERIOR CORNUA OF THE SPINAL CORD.

BY L. C. GRAY, M.D.

The case, whose history I am about to detail, is one that I saw in consultation with Dr. Hughes, who has throughout conducted the treatment, and who has courteously placed the patient at my disposal. The symptoms and their progression have been in most respects so typical of cases of this kind, that I have been led to bring the man before you, as a well-marked specimen of a disease which we have been able to diagnose with great certainty during the past few years.

T. F. C., born in Ireland, drug clerk.

Onset of the disease Sept. 3d, 1879. On the first day there was heaviness of the lower and upper extremities, as if a cannon ball had been tied to the former, and could with difficulty button trousers. On the second day walked with great difficulty, dragging the feet, and letting them fall heavily after being lifted a short distance; could not now button trousers. On the third day could barely descend and ascend the stairs, falling in doing so, and could only hold a spoon by grasping it with the whole hand. On the fourth day could not walk at all, could just lift legs slightly, could not use upper extremities to feed himself, and could not lift himself in bed. On the fifth day was completely paralyzed.

The only premonitory symptom was a feeling of slight lassitude for some three days preceding the attack. No cause known.

Remained completely paralyzed for about three months. At about the beginning of the third month would have cramps in the calves about once a week, to which he had been subject about once monthly during health. These lasted about two or three weeks. On one occasion had a cramp of flexors of left forearm and fingers.

I saw the patient on the eighth day after the onset. There was then marked diminution in the contractility of the muscles and motor nerves in response to faradism. The reaction to galvanism was not tested.

Wasting of the paralyzed limbs began during the first month, and was very well marked at the end of the second.

At no time has there been any, even the slightest, affection of sensation, of the bladder or of rectum; and the mental faculties have been preserved in the most perfect integrity throughout. Nor has there been any febrile condition, except what might perhaps be indicated by the fact that patient had considerable thirst during the first few days.

Improvement slowly began in the fourth month, and patient became able to sit up in bed, raise leg slightly, lift arms, but could not use hands, as to write, etc. In fifth month could raise himself out of bed, and could stand when supported. In the sixth month could walk when supported, and could write when holding the pen in the usual way between the thumb and forefinger, but dragging it after the hand; also noticed slight improvement in the wasting. In the seventh month walked alone around a small room some ten times in the mornings, and wasting is so much diminished that patient is now as stout, or nearly so, as before the illness. In this, the eighth month, he has been able to come here tonight with the assistance of a friend. You see how he walks, although it is with a foot that he has sprained during the last week. You see how his legs are placed far apart to give him a broad base of support, his body bent forward, for he cannot make it perfectly erect, his arms outstretched to balance himself.

Treatment was at first with ergot. fld. extract and potass. iodid.; then the iodide carried progressively to \mathfrak{Z} i. three times daily, and when this amount was reached, the improvement began; faradic electricity daily to trunk and limbs; massage of whole body three times daily; and lately cod-liver oil, iron, quinine, bitter tonics.

This is a case of disease of the ganglion cells in the anterior horns of gray matter of the spinal cord, extending from the cervical to the lumbar enlargement. These cells, as you know, give origin to the motor nerves by means of one long, unbranching process, which becomes continuous with the axis cylinder of the nerve, being known as "the axis cylinder

process." They also regulate the nutrition of the nerve and the tissues to which it is distributed, although it is as yet undecided as to whether there are both motor and trophic cells among them, or whether the motor cell is also trophic.

The diagnosis was made upon a few phenomena. The paralysis was absolutely motor, no sensory impairment of any kind being present.

The reaction of the nerves and muscles of the paralyzed limbs to the faradic current was markedly less than in health. The patient was 22 years of age. There were positively no other symptoms of lesion of the nervous system than that afforded by this purely motor paralysis. This was quite sufficient. There are only two diseases which could have produced this group of symptoms, viz.: a myelitis of the anterior horns and a lesion of a peripheral nerve, interrupting or severing the connection with the multipolar cells of the horn, and therefore producing the same effect as injury to the cells themselves. As it was extremely improbable—nay, unheard of—that there should be a coincident disease of all the peripheral nerves, going to the muscles of the four extremities and the trunk, this supposition could be lightly dismissed, and there remained nothing but the myelitis of the anterior horns to account for what was before us.

Had I tested the muscles and nerves in this case with the galvanic current, I should very probably have obtained what is known as the "reaction of degeneration," on "Entartungsreaction" of the Germans. Let me endeavor to explain this. A healthy motor nerve, with the muscle attached to it, responds in a way of its own to electricity. When a faradic current is applied, muscular contraction ensues, being stronger as the current is stronger, and being produced equally well by either pole. But when use is made of a galvanic battery, there is found to be a great difference in the action of the poles as we increase the strength of the current. With a weak current the negative pole produces contraction at the closing of the circuit; as we add to the strength, the positive pole produces contraction at both the opening and the closing of the circuit; and, finally, with the strongest current, the negative pole causes contraction at the opening of the circuit. All this is altered when a motor nerve is severed, or when the cells of the anterior horn become diseased. The reaction of the nerve and muscle to faradism becomes much less, and may disappear; and instead of the orderly reaction to galvanism as given above, the succession of the poles is reversed. The positive pole will produce contraction at the closing of the circuit with as weak a current as the negative, or even with a weaker one, and the negative pole will contract a muscle at the opening of the circuit before or as soon as the positive. When the diminished farado-contractility is marked, the gal-

vanic alterations are usually present, but not always; so that it is most accurate to test with both currents. When this "reaction of degeneration" is present it is positive evidence of degenerative changes in the nerve and muscle, which are marked in proportion to the degree of the electrical alterations.

This patient has certainly done better than he had reason to expect, and it is fair to attribute this to the careful, persistent and thorough treatment which he has received. He will probably improve for some time to come, but there will remain an incurable residue of paralysis that will finally leave him a much less serviceable man than he was before the attack, although he will be able to earn a living in some of the less active walks of life.

DISCUSSION.

DR. SHAW asked if there was tendon reflex in this case. He had examined several cases of subacute and chronic myelitis of the anterior cornua, and the tendon reflex was absent in each one; he was disposed to believe it was absent in all cases of this disease.

DR. GRAY replied that there was no tendon reflex in this case, and was disposed to agree with Dr. Shaw, for he had examined a number of cases and had always found it absent.

NOTES ON PEPSIN.

BY P. H. KRETZSCHMAR, M.D.

That great differences of opinion prevail among the best of men on subjects connected with theology and on political questions is well known and seems entirely justifiable; that different schools of medicines are in existence is a fact which might be explained in several ways; that the views on Phthisis Pulmonalis are of a great variety has not long ago been shown by a learned discussion held in this room; but how shall we account for the wide difference of opinion which exists among careful observers about the value of "Pepsin"? Of all the preparations used in medicine there is hardly one which is regarded so differently by equally good men; considered worthless by some, esteemed to a high degree by others.

To support the statements just made, allow me to quote a few of the replies which I received from the members of this Society and other gentlemen to certain inquiries about Pepsin:

Dr. E. N. Chapman says:

"When Pepsin was first introduced into practice a number of years ago, it seemed to me that it would be a valuable agent in certain enfeebled conditions of the stomach. Consequently I tried it faithfully and persistently, using first the American and then the French. At last, when I could not convince myself that much advantage was gained, I resolved to experiment with nursing babies, in whose passages there were habitually small masses of caseine. In such a case the Pepsin ought to complete what the gastric juice had failed to do. Alas! for theory; it invariably increased the indigestion. Therefore, having found to my satisfaction that Pepsin was useless, I have not prescribed it the last ten years. To conclude, I would say, every man, woman and child must manufacture his own Pepsin in order to secure a kind both reliable and efficient; all others are worthless."

Dr. W. H. Bennett states, that he does not use Pepsin, because he gets just the same results without it, and his opinion about the relative value of the different preparations is, that they are all about alike and in his experience useless.

Dr. W. Wallace uses Pepsin only to a limited extent, owing to the fact that for some cause or causes he did not get the beneficial effect looked for.

Dr. J. D. Sullivan has some years ago used considerable Pepsin, but he became dissatisfied with the effects it produced and uses it now but little.

Dr. C. L. Mitchell employs Pepsin frequently and freely. More so formerly than now. The doctor has "not much" confidence in it.

Dr. H. Hardrich has used Pepsin in about ten cases of functional disorders of the stomach with negative result.

Dr. R. Hesse does not prescribe Pepsin as often now as he did formerly; he thinks that several of the preparations sold are inert.

Dr. Geo. A. Ostrander employs Pepsin frequently, but he remarks that he has nearly lost faith in all of the various preparations.

Dr. B. F. Fessenden has never seen any good results from the administration of Pepsin.

Against these adverse opinions there stand about forty-eight expressions of recognition of the value of Pepsin; while fourteen answers state that the writers did not use Pepsin to any extent.

In answer to the first question, Do you use Pepsin to any extent, beside the plain statement of "Yes," the following replies were received:

"I constantly prescribe it."

"Have used it frequently, consider it a most valuable agent."

"Use it largely among children, very frequently among adults."

"I do, very largely."

"Very frequently."

"Largely in the summer."

"Yes, quite largely."

"Considerable during the summer months."

In looking over these widely varying opinions it seems almost natural to try and find some way to solve this mystery.

If those who regard Pepsin as a worthless addition to the list of recognized pharmaceutical preparations are right, then all those who prescribe it, are, to say the least, not good therapeutists; and again, if Pepsin is a valuable remedy, those gentlemen, who omit to prescribe it, do not give their patients the full benefit of scientific treatment.

Perhaps some light might be thrown on this very important subject by investigating the following questions:

A.—How should Pepsin be administered?

B.—In what class of cases might Pepsin be used, according to the laws of “physiological antagonism”?

C.—How can we detect which of the numerous preparations in the market will give the best results?

A.—*How should Pepsin be administered?*

If Pepsin is dissolved or suspended in any fluid of neutral reaction its power of digestion is interfered with; that means, it cannot show its full strength. If, however, the solution is made alkaline, the Pepsin at once becomes inert—it loses all power to change albumen into pepton. The proper relation between some kind of acid—hydrochloric or lactic to be preferred—and the Pepsin is an essential condition to bring out its digestive power, and, in therapeutics, the good effects of Pepsin. Not only as an exception, but we well might say as a rule, we find in looking over prescriptions which contain more or less of Pepsin, that the practitioner tries his best to combine Pepsin with a variety of vegetable or mineral substances in such a manner that it cannot show its digestive power at all. Nothing is more common than to see Pepsin combined with subcarbonate or subnitrate of bismuth, and yet, it can easily be shown that the addition of even the latter salt to artificial gastric juice will interfere with the digestion of egg albumen. Bicarbonate of soda, the different preparations of iron, strong alcoholic tinctures, and elixirs are incompatible with Pepsin.

For some reason or another, however, these combinations are ordered by professional gentlemen of high standing.

For an illustration of the tendency to combine Pepsin with other drugs in an almost ridiculous manner allow me to quote from a circular issued by Carl L. Jensen, M.D., Vermillion, Dakota, on the merits of his own preparation of Pepsin.

The doctor suggests among others the following combinations:

Castor oil emulsionized with Pepsin:

R. Pepsini, ℥ii.; aquæ cinnamom, ℥v.; liq. potassæ, ℥i. Tere bene et adde, ol. ricini, ℥ss. Fiat emulsio. Adde tr. opii, ℥i.; glycerini pur., ℥i.; aquæ cinnamom, q—s—AD ℥iv. S. Small teaspoonful every two or three hours. A good cholera infantum remedy. (?)

Dr. Jensen also gives a formula for the preparation of an “Elixir of iron, quinia, strychnia and Pepsin,” which reads as follows: R. Quin. sulph., gr. 64; strychn. pur., gr. i.; acid, citric, gr. x.; alcohol, fort., ℥iv.; spir. aurant, ℥i.; Pepsini, gr. 256; syr. simplic., ℥vi.: ferri pyrophosphate, gr. 256; aquæ pur., ℥v. Misc. et solve.

As said already, these formulas are only given to show the general tendency to administer Pepsin in the worst possible form, and it is really not necessary to go so far as to look for evidence to Dakota. Almost every one of the large number of "manufacturing pharmacists" which supply the market with a thousand and one elixirs, syrups, wines, etc., is making very similar mixtures. An exception of the rule may be mentioned right here. Schieffelin & Co., New York, in their latest lists of goods, say of their elixir of iron, quinia, strychnia and Pepsin, that they make this article simply to supply the demand; that they, however, consider the preparation a superfluous one.

There seems to be, of late, a waking up of the medical profession as to the abuse just mentioned. I allow myself to call your attention to the following contributions on the subject, written by representative men:

* "A second illustration both of legitimate and illegitimate contributions made by the pharmacists is to be found in the history of Pepsin. The foreign Pepsins were sure to be expensive, inelegant and disagreeable, and sometimes, if not always, inert. Mr. E. Scheffer, of Louisville, has invented a very simple method of preparation, which subjects the Pepsin to but few disturbing operations, and brings it into a form suitable for easy and elegant dispensing. He has also gone further, and illustrated by a careful series of experiments the method of its action, showing it to be a ferment rather than a solvent, and capable of converting into peptones, under appropriate conditions, a quantity of albumen many times greater than that which it is supposed to dissolve in the ordinary experiments. He also showed that certain substances presented with it made it inert. Alcohol diminishes, and if in sufficient quantity, prevents its activity. In an alkaline solution, decomposition and putrefaction takes place, but no digestion. Bismuth has also been shown to be incompatible therewith, and yet nothing is more common than to see elixir of Pepsin, bismuth and strychnia, which darken, harden and shrink the albumen placed in them, and which seem to render visible a condition of acute dyspepsia.

"Pancreatine has been shown by Mr. Scheffer to be itself digested by Pepsin, so that its chance of getting through the stomach to the duodenum, where it normally exerts its function, is of the smallest; while, when combined with Pepsin, it must be digested as soon as the mixture becomes warm enough, in or out of the stomach, to carry on the process. Yet we see many preparations of which the chief virtue is supposed to be that they contain *all* the digestive principles. These can be active, it

* Boston *Med. and Surg. Journal*, Jan. 15, 1880. Robert T. Edes, M.D., Prof. of Mat. Med., Harvard University.

seems to me, only so far as they contain Pepsin, and have no advantage over the simple drug prepared as it now is, by Mr. Scheffer's process, by a large number of pharmacists all over the United States."

Further:

* We took two test tubes, and put in each the same quantity of artificial gastric juice, *i. e.*, a 2% mixture of hydrochloric acid with Pepsin, and then placed in each a few pieces of coagulated albumen. Into one was also placed 3i. of elixir of iron and quinia. Both test tubes were kept at blood heat for ten hours. In four hours the albumen was rapidly disintegrating in the gastric juice alone. At the same time the albumen in the tube containing gastric juice and iron was apparently intact. In ten hours the albumen in the gastric juice was entirely dissolved, while in the other tube it was still intact. The tendency, both on the part of prescribers and the large drug manufacturers, is to combine iron with other tonics, so that the market is flooded with elixirs, syrups and wines of iron and quinia, iron and strychnia, iron, strychnia and Pepsin, and so on ad infinitum. "The combinations with Pepsin are a shameful waste of this valuable medicine, and well calculated to bring it into disrepute."

Further :

† Several facts which I published in my essay (*Am. Journ. Pharm.*, Feb., '72,) impressed on me the impossibility of a preparation, such as elixir Pepsin and bismuth (strychnia). I do not want to speak again about the presence of alcohol in a solution containing Pepsin, as I have repeatedly given the results of my experiments, which prove beyond doubt that Pepsin and alcohol, particularly when the latter amounts to a certain percentage, are incompatible. The main object I intended to bring against such an elixir, prepared with ammonio-citrate of bismuth in a neutral or alkaline proportion, is the neutral or slightly alkaline state. My experiments prove clearly that Pepsin, in neutral solution, does not keep, and that in alkaline solution it loses its digestive properties. Further on, Mr. Scheffer shows that the physician might wish for a combination of Pepsin and bismuth in the liquid state; but another question is, can such a combination be made, or can it exist? Those that manufactured the elixir of Pepsin and bismuth were satisfied to know that they used Pepsin in its preparation; but whether it was in it or in an active form never troubled them, as they never tested for it. They could conscientiously put their label on the bottle and maintain that they used Pepsin in its preparation. The physician prescribes it in good faith, because he has confidence in the firm who makes it, and in the name by which the article is designated.

Having tested several elixirs of Pepsin and bismuth that I could get hold of, I found that even after the addition of hydrochloric acid not the least quantity of albumen was dissolved. After describing his method of testing these elixirs, and speaking of the negative results obtained, Mr. Scheffer closes his paper, saying:

"Abstracting, therefore, from the alcohol, and not speaking of the neutral or alkaline solution, the elixir of Pepsin and bismuth is an incongruity; and when patients have derived any benefit from it, it was from the bismuth it contains and the stimulating effects of the spirits; but surely not from Pepsin, as it does not contain any Pepsin."

* San Francisco *Western Lancet*, Feb., 1880. A. W. Perry, M. D., Prof. of Chemistry, Med. Dep. University of California.

† *American Journal of Pharmacy*, 1872, August, by E. Scheffer, Louisville.

So far, I have spoken principally of certain methods by which Pepsin should not be administered; the question "A," however, is, How should pepsin be administered? *Pepsin acts best and surest when given by itself in acidulated water, with, perhaps, the addition of some pure glycerine.* The dose should not be too small; the acid used should be the officinal preparation of diluted hydrochloric acid, and the medicine should be given rather freely diluted with water. An excellent formula is the following one:

R. Pepsini saccharat, gr. xv.; Acid hydrochlor., dil., gtt. xv.; Glycerini, pur., ℥ii.; aquæ, ad., ℥ii.

S.—Take after meals.

The use of "meat pepton" or "milk pepton" is certainly indicated in certain conditions of deranged stomach digestion; but the discussion of this interesting subject must be left for a future time.

B. *In what class of cases might Pepsin be used, according to the law of physiological antagonism?*

The use of Pepsin should be limited to those cases where, after careful investigation, it has been shown that the difficulty arises from either the presence of indigested animal food in the stomach, or from a perverted condition of the gastric juice, including, under the first class, the numerous cases of diarrhoea of young infants dependent on the presence of indigested food, which occur during the hot season. A deranged condition of the nervous influence interferes materially with the production of healthy gastric juice, and Pepsin, in combination with hydrochloric acid, is indicated in such cases too. But it should not be forgotten that the regulation of the patient's diet in cases of dyspepsia is of much greater importance than the administration of any kind of medicine. Pepsin, if administered in a proper form, and if of good quality, *will and must*, if combined with the necessary dietetic management, assist nature in performing the highly important function of stomach digestion. To prescribe Pepsin, and expect great benefit from its use in cases of acute or chronic gastric catarrh, in gastric ulcer, in gastric disturbances depending on Bright's disease of the kidneys, in cases of interference with the portal circulation, in habitual constipation, or in cases of diarrhoea depending on lesions of the intestinal track, helps just as much to bring the remedy into disrepute as the use of preparations which are of an inferior quality, or its administration in combination with incompatibles.

C. *How can we detect which of the numerous preparations in the market will give the best results?*

Various tests for the relative value of the different kinds of Pepsin have been advised from time to time. The most important ones of those known to me are:

The test by coagulation of milk;
The test by digestion of raw beef (chopped);
The test by dissolving blood fibrine;
The test by dissolving coagulated albumen;
The so-called chloride of sodium volumne test; and last, but not least,

The clinical test.

Clinical experiences have been given in the first part of this paper, not exactly such in regard to the relative value of the different makers' preparations, but more in regard to the therapeutical value of Pepsin in general.

The first test is probably the most uncertain one, while the second and third demand a great deal more care and time than the albumen test, which now seems to be the test most generally adopted (Pharm. U. S., Pharm. Germ.)

The so-called chloride of sodium test is a new one, and if I am not mistaken, it was brought to life by Dr. John Merritt, a member of our Society—a gentleman who has spent considerable time and work in investigating the subject of testing Pepsin.

The test for the quality of Pepsin, which is described in Dr. Merritt's paper "A Ready Method for Testing Pepsin," appears at first sight not only very convenient, but also quite conclusive. Unfortunately for us, however, this test does not altogether agree with the physiological test—the power of transferring albuminoids into peptones; and if the same preparations which, by the addition of salt solution, allow the largest amount of condensed substance to rise to the surface, do not in the same proportion transfer the largest amount of albumen into peptone, Dr. Merritt's—or, as it is sometimes called, "the Hawley test"—is unreliable and worthless. The precipitate caused by the addition of saturated solution of sodium chloride can be increased from two causes, but both these causes would rather interfere with the physiological properties of Pepsin, instead of adding to the strength of the preparation.

First, the Pepsin might contain a great deal of mucus, which would show itself by great turbidity, when the Pepsin is brought in solution; afterwards by the precipitation with salt, the mucus will adhere to the Pepsin precipitate, and increase its bulk considerably. There was a time when digestive action was ascribed to the mucus, and the mucus, being scraped from the mucous membrane, was simply tried and used for its digestive properties, because it contained Pepsin. But who will claim now any digestive properties for mucus; more than that, who will deny

that putrefied mucus interferes materially with the proper action of Pepsin? It may be stated just here that Pepsin, which possesses more or less of an ammoniacal odor, should always be used cautiously, for the reason mentioned, that putrefied material introduced into the stomach, and more especially in cases of gastric disturbances, will prevent instead of assist the physiological action of that organ.

The second cause for a large precipitate might be pepton. When by the preparation of Pepsin the stomachs are left too long in contact with the acidulated water, into which they are placed for extraction, particularly in very warm weather, the dissolved Pepsin acts as solvent on the mucous membrane of the stomach, and forms thereby pepton, which is also precipitated by the sodium chloride, and helps to increase both the volume and the weight of the precipitate, without adding strength to the Pepsin.

Finally, it may be stated that the test of Dr. Merritt seems to prove itself unreliable by comparing the relation between the volume of the precipitate and its weight. Dr. Merritt himself has kindly allowed me to use the precipitates which he obtained during the last series of his experiments, and the following is a comparative statement between the volume—as given by the doctor—and the weight as obtained by me, through the kind help given me by my friend, Mr. H. Syvarth, of the firm of Syvarth & Schleussner, 145 Atlantic Avenue, of this City :

No.		Volume.	Weight.
1.	I. Hawley	38	$1\frac{1}{10}$ gr.
2.	McKesson & Robbins	1	$\frac{4}{10}$ gr.
3.	Wm. Proctor & Co.	3	$\frac{7}{10}$ gr.
4.	Wm. H. Schieffelin & Co.	5	2 gr.
5.	Lazell, Marsh & Gardner.	2	$\frac{1}{2}$ gr.
6.	E. Scheffer	8	$3\frac{7}{10}$ gr.
7.	Kidder & Laird	3	$\frac{1}{2}$ gr.
8.	G. H. White	0	$\frac{2}{10}$ gr.
9.	Hance Bros. & White	4	$\frac{6}{10}$ gr.
13.	J. Wyeth & Bros.	0	$\frac{1}{2}$ gr.
15.	Smith & Pitkin.	8	$\frac{9}{10}$ gr.
17.	Lactopeptine	4	$1\frac{2}{10}$ gr.

The original test shows plainly enough that the precipitate of Pepsin No. 1 is much more voluminous than any of the others; as a matter of fact it is just as large as all the others put together, but, after carefully separating the precipitate from the liquid and drying it, it appears that the deposit obtained from No. 6 Pepsin weighs three times as much as that from No. 1. Crystals of chloride of sodium can easily be detected in the dried precipitates, and from what has been said, I think it may be concluded that the chloride of sodium test, as proposed by Dr. Merritt in his interesting paper, and by Dr. Hawley in his advertisements, is unreliable and worthless.

The only proper method for testing the digestive power of Pepsin is the albumen test, if it is made in a careful manner. To say that 10 grs. of Mr. X.'s Pepsin will dissolve 500 grains of egg albumen, means but very little; it is of vital importance to state the exact proportions of Pepsins, albumen, acid and water; for 10 grains of the former may transfer 500 grains of albumen into pepton in 4 ounces of acidulated water, while probably half as much more could be acted upon if placed into 8 ounces of acidulated water. The test, as given in the report of the committee of the American Pharmaceutical Association on the revision of the U. S. Pharmacopœia, does not seem to give justice to Pepsin; the report says: "The strength of the product should be so adjusted that 10 parts of it, dissolved in 150 parts of water and three parts of hydrochloric acid, will dissolve at least 120 parts of egg albumen at a temperature of 40° C. (104° F.) in five or six hours." Ten parts of good Pepsin should dissolve a much larger amount of albumen, if placed in a larger proportion of acidulated water, which would also be more like the relative proportions of Pepsin and acidulated fluid, as found in normal gastric juice—15 parts of Pepsin to 975 parts of water.

The following table shows the result of carefully made experiments according to a method proposed by Dr. H. Hager, in his latest and greatest work, "*Handbuch der Pharmaceutischen Praxis*," and indorsed by Prof. E. Scheffer, of Louisville, Ky., in a letter written to me a short time ago. The tests have been made with the greatest possible accuracy in the laboratory of Messrs. Syvarth & Schleussner, under the personal supervision of both Mr. H. Syvarth and myself. Great care has been taken to treat all samples alike in every respect—the same advantages which might have affected the one were given to all of them, and vice versa. The eggs were all boiled at the same time, afterwards cooled, freed from all membrane and yolk, and cut into as small particles as practicable. These were thoroughly mixed and the quantity placed in each vessel was carefully weighed. All the vessels were of exactly the same size, placed in a water-bath of a temperature of never below 90°, and never above 102° F., and agitated equally often. None of the samples were received from the manufacturers. The test recommends itself on account of its simplicity, and, according to my own conviction, accuracy. We took 0.1 Pepsin; 10.0 hard-boiled egg albumen; 100.0 water; 2.0—chem. pure hydrochlor. acid; so that one part of Pepsin acted on one hundred parts of albumen in one thousand parts of (2%) acidulated water.

We kept the mixtures until some one had dissolved all the albumen; one part of Pepsin had dissolved 100 parts of egg albumen, and at that time we removed all the bottles from the water-bath. We then strained the different residues, washed the remaining albumen off with a small

amount of water, and after slightly drying it between filtering paper, we weighed the undissolved albumen

We next started to make the following calculation: The sample which dissolved all the albumen we call a 100% Pepsin, and every one else in proportion. For instance, if the residue would be 5.0, the sample be called a 50 % preparation, or if 3.5 only remained undissolved, the article is a 65% one, etc. Not before 36 hours after the Pepsin was allowed to act upon the albumen, we found one sample which had digested the full amount given to each one. Not to mislead any one, and to show fair play to every party interested, I give below the result of our investigation only—I omit any reflections, and I simply mention the wholesale prices of the different preparations, as to allow a fair comparison.

The table will explain itself after what has been said:

Name of Manufacturer.	Amt. dissolved of 10 grammes of Egg Albumen.	Percentage of Pepsin.	Price of Pepsin.
E. Scheffer, concentrated or pure.....	10.	100%	\$4 00
Bullock & Reynolds, London, known as "Beale's" ..	9.1	91%	4 00
J. Hawley's sacch.....	9.0	90%	45 cts.
Phoenix Chem. Works.....	9.0	90%	1 00
Böenz, Good & Co.....	8.5	85%	1 75
Boudcault (starch).....	8.5	85%	85 cts.
E. Scheffer's sacchar.....	7.7	77%	60 cts.
Smith & Pitkin, Syracuse.....	6.7	67%	
Kidder & Laird, sacch.....	6.3	63%	35 cts.
W. H. Schieffelin, sacch.....	6.2	62%	
Lehn & Fink, pure.....	6.2	62%	90 cts.
Armstrong's sacch.....	6.1	61%	35 cts.
Carl Jenson's pure.....	5.5	55%	1 50
Austin's sacch.....	5.4	54%	35 cts.
Lactopeptine.....	4.9	49%	70 cts.
Hance Bros. & White.....	4.5	45%	
Lazell, Marsh & Gardiner.....	3.4	34%	
McKesson & Robbins.....	3.0	30%	
Morsen Peps. Porci', pure.....	2.3	23%	3 40
Ingluvin.....	1.7	17%	1 00
John Wyeth & Bros.....	1.5	15% ..	66 cts.

After the foregoing table was completed I saw an article on Medical Pepsin and Artificial Digestion in the last number (March, 1880) of the *London Practitioner*, written by Dr. G. F. Dowdeswell. The doctor publishes there two comparative tables, and although his experiments are more or less confined to preparations which are made in England, those which were tested by both of us show very similar results. He found Bullock & Reynolds' (Beale's) Pepsin the best, and Ingluvin he places with Wyeth's, at the foot of the list.

Drawing conclusions from these notes, they might be formulated in answer to the three questions raised:

A. Pepsin is frequently prescribed in such a form as to interfere materially with its valuable properties—it should always be given by itself, in acidulated water.

B. Unreasonably good results, in a variety of diseases, are claimed for Pepsin by the laudations of certain individuals—its good effects are limited to certain functional disturbances of the stomach.

C. Many of the preparations sold are of an inferior quality. The physician should be careful to select a Pepsin which not only bears that name, but possesses some digestive powers.

For the Discussion on this Paper, see the Minutes of the Society's Stated Meeting, April 20th, 1880.

A READY METHOD OF TESTING PEPSIN.*

BY J. MERRITT, M.D.

There has been much said and written on the subject of Pepsin. Many physicians have abandoned using it on account of the frequent failure to obtain satisfactory results; in other hands, Pepsin has proved itself not only valuable, but almost indispensable. These differences may, I believe, be explained by the great variation in quality of the article, as found in the market.

The object of this paper is to suggest a ready method for estimating the amount of Pepsin present. To this I have appended the results of several observations, made by me, upon some of the best known preparations of Pepsin.

The test is an adaptation of E. Scheffer's process for manufacturing—that is in general use in this country. The process was originally described in the *American Journal of Pharmacy* for 1872. "The United States Dispensatory" (14th edition, p. 1745), under the head of "Rennet," gives the process as follows: "Mucous membrane of the pig's stomach, dissected off and finely chopped, is macerated in water acidulated with muriatic acid for several days, with frequent stirring. The strained liquid, if not clear, is clarified by allowing it to stand for twenty-four hours and decanting. The same bulk of a saturated solution of chloride of sodium is then thoroughly mixed with it. After several

* Read at Dec., 1879, Meeting of the Society.

hours the floating Pepsin is skimmed from the surface and put on a cotton cloth to drain, and finally submitted to strong pressure to get rid of saline solution. This Pepsin, when air-dried, is very tough, parchment-like or leathery, varying in color from a dim straw yellow to a brownish yellow. To make his *sacchrated Pepsin*, Mr. Scheffer adds sugar of milk until a powder is obtained, 10 grains of which will dissolve 120 grains of coagulated albumen." The "National Dispensatory" (1st edition, p. 1040) details the same process.

For the estimation of the amount of Pepsin present, put ten grains of the sample in a test tube, not less than 1 in. in diameter, add about one ounce of water, agitate frequently; when solution has occurred, add an equal amount of a saturated solution of table salt (sodic chloride); allow to stand for 24 hours; the Pepsin will be found floating in the upper part of the fluid. The quantity may be conveniently marked by a slip of paper, with the name or initial of the manufacturer thereon, cut to the same depth as the floating stratum, and pasted on the tube.

Other specimens may be so treated, the results recorded in a similar manner, and thus a sufficiently accurate comparison made for all practical purposes.

This cut represents a tube so prepared as to illustrate my meaning.

Seventeen specimens are included in my observations.

All were purchased in original packages, with the exceptions of Beales' and Savory & Moore's.

The five series marked "A" were with 30 grains of the powder, in tubes $1\frac{1}{4}$ inches in diameter; in series marked "B" the tubes were $\frac{3}{4}$ of an inch in diameter; 10 grains were used of all, except No. 1, (Hawley's), 5 grains of which were employed. The larger quantity, it was found, would not rise in a tube of this size, but adhered to the sides. The figures, in these cases, have been multiplied by 2, to make them conform to the 10 grains of the others. In Series No. 6, 1 gramme of each.

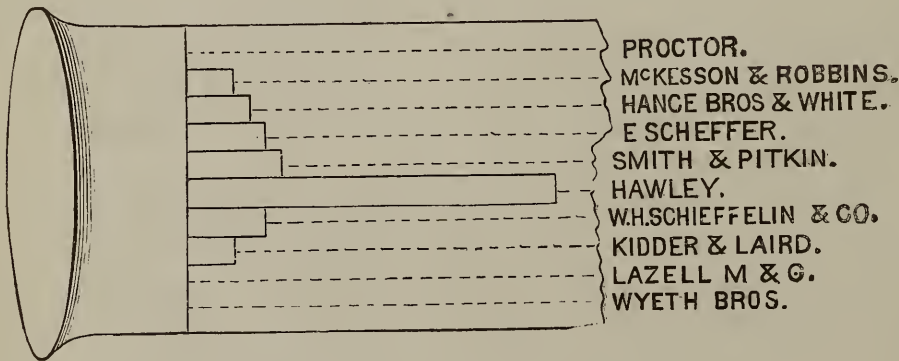
The following table exhibits the result:

The figures in the columns show the depth of the floating Pepsin, expressed in sixteenths of an inch. The reaction of all the solutions except Hawley's and Boudault's starch, was neutral or alkaline. Boudault's starch gave a ppt. after neutralization with ammonia. Boudault's sacch. and Witte's failed to give the ppt. Beales' and Savory & Moore's are the scraped mucous membrane, and contain mucus and epithelium. So much gas was generated by decomposition as to interfere with the recording of results. These five were therefore omitted after the first series. Lactopeptine was added after the sixth series. Series No. 11 was made with water acidulated with hydrochloric acid. J. Wyeth & Bro.'s new Preparation, "Pepsin Porci," shows 3-16 in. for 20 grs. My supply was insufficient to include it.

NO.	NAME.	SERIES A.					SERIES B.					
		I	2	3	4	5	6	7	8	9	10	11
I.	J. S. Hawley	23	24	32	24	24	23	20	28	24	38	38
II.	McKesson & Robbins	1	3	3	1	2	1	0	1	0	0	1
III.	Wm. Proctor & Co.	0	2	1	1	1	0	0	0	1	0	3
IV.	W. H. Schieffelin & Co	2	5	5	6	9	11	4	6	5	4	5
V.	Lazelle, Marsh & Gardner	0	2	0	2	0	0	0	1	1	0	2
VI.	E. Scheffer	6	5	4	6	3	5	2	2	3	2	8
VII.	Kidder & Laird	2	3	3	2	7	5	5	4	4	3	3
VIII.	Geo. H. White	0	1	0	0	0	0	0	1	0	0	0
IX.	Hance Bros & White	2	4	2	3	3	4	2	2	2	2	4
X.	Boudault (starch)	0
XI.	" (sacch.)	0
XII.	Beales	0
XIII.	John Wyeth & Bro	0	0	0	0	0	0	0	0	0	0	0
XIV.	Smith & Pitkin	4	6	5	6	7	7	6	6	4	4	8
XV.	F. Witte
XVI.	Savory & Moore..
XVII.	Lactopeptin	5	5	4	4	4

It will be seen that this test is qualitative, and not quantitative. To separate, dry, and weigh the precipitates, would require more attention and apparatus than the general practitioner has at his disposal. The salt solution appears to coagulate the Pepsin, and the amount is estimated by the eye, as in the case of albuminous urine.

Perhaps these facts may explain the frequent failure of preparations of the digestive principle. By this simple test the presence or absence of Pepsin may be determined.



[The foregoing paper was read by Dr. J. S. Hawley, who explained that Dr. Merritt had been prevented from attending this meeting, and had, only a few hours before, come to him and requested him to present the paper.]

DISCUSSION.

DR. E. R. SQUIBB said that the only remark which occurred to him was, that while the Pepsin in the tubes was being liberated slowly, the particles took irregular star-like shapes, the angles and projections of which allowed them to approach each other more or less closely under slightly differing curvatures. That is, as the Pepsin is liberated, it goes to the top of the liquid in a more or less compact form. Now the thickness of this layer on the top of the liquid in the tubes has to be judged of in regard to its density as well as volume, and layers of equal thickness may be very different in density and compactness of structure, and therefore in their quantity. This has been alluded to by the author of the paper when he says that it is a qualitative and not a quantitative test of the Pepsin. Even if allowed to stand twenty-four hours, equality in density could not be assured.

DR. GEORGE K. SMITH asked the reader of the paper (Dr. Hawley) whether the weight of substance at the top of the tube bore any relation to its apparent thickness; would it weigh a good deal less when compact than if it were more diffuse. Dr. Hawley had been in the habit of waiting until the liberated mass was as nearly compact as possible before he weighed it. He had in his laboratory the results of a series of experiments which he had performed long before those performed by the author of the paper, in which he weighed the precipitate, and he found the weight nearly in proportion to the bulk occupied, after twenty-four hours had passed. He supposed, therefore, if these tubes were allowed to stand here until to-morrow evening at ten o'clock the Pepsin contained in them would be as nearly compact as it is possible to get it, and the weight would not be materially different from the space occupied by that substance proportionately. Some of the "Pepsin" experimented upon is from the most popular manufacturers in the United States; and yet, as is evident, there is but little or no Pepsin there.

DR. SANFORD mentioned a case in which a preparation of Pepsin was given for a long time without benefit. Afterwards Pepsin from another manufacturer was taken, and complete recovery took place. He thought it possible that he might at first have taken a specimen in which there was no Pepsin, and afterwards a specimen in which there was Pepsin; and this may account for the recovery.

DR. WALKER suggested the possibility of Pepsin changing very rapidly by decomposition. That may be one reason why good results are frequently not obtained.

DR. SMITH inquired whether there was any reason, other than the want of a proper quantity of Pepsin, why the weight of the mass at the top of the liquor should not be in proportion to its thickness?

DR. HAWLEY had read in the paper Mr. Sheffer's method—which is universal among manufacturers in the United States—for extracting Pepsin. He was not aware that any other was employed. It claims to put freshly precipitated Pepsin in sugar of milk, and grind them together. Consequently, if we take these specimens, we assume that they have pursued this course. Some put in more and some put in less. That is one way to account for it.

A pharmaceutical manufacturer in New York city, largely engaged with Pepsin, employed men to prepare it in the basement of the slaughter-house. This became known to the butcher boys, who soon became acquainted with the method employed, and the foreman of the slaughter-house says he knows that the butcher boys, on their own responsibility, made the preparation from the results of their maceration of pigs' rennets. Now, every one knows that pigs' stomachs contain a great deal of mucus, and these persons have not the knowledge, and perhaps not the desire, to separate it.

The fact that these men do make these precipitates, and do sell them to manufacturers, is not a surmise on his part. This may account for the total want of Pepsin in some of the specimens.

THE CHAIR stated that he desired that names should not appear in the record of this debate, although it was important these facts should. Names might be retained in the memory.

DR. SMITH thought that they should not only be in the head, but these things, names and all, should be on record. If such things are practiced, those who are most interested should know it. For it is important that pure articles should be furnished, and it is of equal importance to know where they can be purchased, as well as to know where inferior articles are sold. In times gone by it was difficult to secure good articles of medicine; but improved knowledge and information has improved this, and physicians desire to continue this progress. If physicians could find a really good article, they want to know it, and where it can be found.

DR. HAWLEY wanted it distinctly understood that the firm referred to did not manufacture this "mucous Pepsin;" the boys took advantage of the opportunity afforded, learned their process, manufactured this stuff, and sold it to others. Of course, there was no responsibility on the part of this firm; but the fact was mentioned to explain in a measure the reason for the presence in the shops of an inferior article.

ON THE RELATIONS OF THE MEDICAL PROFESSION TO THE TRADE INTERESTS OF THE MATERIA MEDICA.

BY E. R. SQUIBB, M.D.

It is hazardous to the best interests of both the science and the art of medicine for medical men, either individually or through their organizations, to publish anything that can be used to advertise individual or proprietary specialties for business purposes.

The medical journals of the day owe their existence as such very largely to the pecuniary support obtained from the advertisements upon their fly leaves; and when these advertisements can be made to pay the advertisers better by the use of names or societies, or by references to published papers, such uses and references are very likely to be made.

Judging by their actions, a very considerable proportion of the medical profession consider such uses of their names and their papers entirely legitimate. Another proportion give their names and write their papers without thought as to whether they can or will be so used, or not; while another small proportion appear to give their names and write their papers especially for advertising purposes.

Those whose names and papers are so used become at once special partners in business enterprises, or endorsers of speculative business paper; and if this fact could but be recognized by all readers of such names, papers and advertisements, the risk of harm would be much less. But, unfortunately, the relation is not generally recognized in the medical profession, though very promptly recognized and utilized by the mercantile or trade interest involved. Hence, when the medical man joins the business man in extending the profits of his business, the medical man necessarily joins in the risks and liabilities of the business, and loses in reputation and professional tone and dignity all that the business man gains by him, while the business man gets all the money. What the medical man expects and hopes to gain by lending his name and influence is a good specialty for the use and benefit of his profession at large. And in his hope and through his enthusiasm he disregards the experience of both past and present time, throughout which fallacies and quackeries have passed and are passing into disuse, despite the physicians' certificates and endorsements, and so-called experience in their use, dragging down with them the names of their endorsers. The business man, however, does not go down with his wares and his doctors' names, but having harvested his profits, invests a part of them in new enterprises.

It was wonderful that Swain's Panacea, Cundurango and Missisquoi waters could each in turn repeat the history of its predecessors, and that hundreds of other articles could keep the line of succession unbroken; but it is still more wonderful that upon the testimony and by the support of the medical profession, through its advertising tendencies, the number of such specialties constantly increases. The endorsed statements of to-day in regard to the various foods for infants—Pepsins, Chloro-pepsins, Lactopeptin, Ingluvin and Pancreatin—if accepted, and if only enough of them were bought, would leave the condition of indigestion out of the list of human ills. Add to the perfect digestion and assimilation thus insured to young and old the endorsements of the tonic series of Beef, Wine and Iron, and the Elixirs, and there is hardly anything left to wish for except the endorsements of Damiana. Bladder-wrack or Anti-fat might indeed be needed. But how there should be any consumption left for Benzoate of Sodium to cure, or any of the long list for Pond's Extract and Radway's Ready Relief, Holman's Liver Pad, etc., cannot be logically understood. Yet it is not at all doubtful that all this long line of specialties and quackeries owes its unbroken succession, and the proprietors owe much of their money to the advertising and the support obtained from the medical profession.

Let any one interested in this matter—and what physician is not?—glance over the testimonials wrapped round the sample bottles left by the

ubiquitous and irrepressible drummer, without thinking so much of the substance endorsed as of the validity of human testimony as evidence; and by the time he has glanced over two or three rival circulars he must conclude that the relation between statements and facts is not materially altered by such testimony—that florid assertions are mere assertions still, and are yet as far from being established truth as if not supported by prominent names in the medical profession.

Some look to a higher standard of medical education to correct this evil. But if such will glance again at such testimonials, they will find the names of many men whose qualifications, and whose standard of medical education, are far above any general standard that can ever be reached—names of leaders and teachers high in the profession, and subscribed to patented specialties under such statements as “The most important remedial agent ever presented to the profession.”* A higher standard of medical education is undoubtedly needed, and this will help to correct the practice by teaching the value and the character of accuracy in research before statements are made. But is it not rather a higher standard of moral responsibility—of respect for the truth—that is needed here? The statements themselves, in the abstract letter, may be truthful—at least in the sense of being believed by those who make them—and yet the purposes with which they are given, and the uses to which they are put, make them generally untruthful and hazardous.

Leaving out of consideration that small proportion of the medical profession which purposely lends itself outright to the making and advertising of specialties as a business, the main cause of the evil is, perhaps, that physicians will accept any testimony as evidence, and thus allow themselves to act upon insufficient grounds. For example, if a physician testifies to a special make of pepsin, there are several prominent reasons why his testimony may not be safe as evidence. First, if his testimony be definite and florid enough to be of much value to the manufacturer, it is almost impossible that it should be accurate enough to be accepted as evidence. He cannot be sure that the sample tested accurately represents the whole product of the maker. He cannot be sure of his methods and conditions of testing, without a carefulness and thoroughness of research and observation which is rarely attempted in such cases, because rarely considered necessary, and finally, he cannot be sure that the article, at his time of testing or using, is of the same quality as it will be in the future, when his testimony and endorsement is used for it. The testimony remains unchanged; but, admitting it to be accurate-

*See cover of “The Practitioner—A Journal of Therapeutics and Public Health,” for February, 1880,

ly true and just, the substance endorsed may change indefinitely. Other reasons why certificates and endorsements of specialties in the materia medica are dangerous may be seen from the following example:

On the notice of the December meeting of this Society, it was announced that a member would read a paper on "A Ready Method of Testing Pepsin." When the paper was called for at the meeting the author was not present, but the paper was offered and read by another member, who is a prominent manufacturer of pepsin. The test described in the paper is made by precipitating in a test tube one inch wide, the pepsin from ten grains of the sample, dissolved in "about one ounce of water," by means of an equal volume of a saturated solution of common salt, allowing the whole to stand 24 hours, and then measuring the thickness of the precipitate floating on top, and counting the thickness in sixteenths of an inch.

In the reading of the paper it appeared that the pepsin of sixteen of the more prominent manufacturers, including that of the reader of the paper, had been submitted to this test many times, and that the pepsin of the reader of the paper was, on an average, more than ten times better than that of the other makers. The names of all the sixteen makers were given, and a table and wood-cut were shown comparing the makers, and with the names attached. In the table, which showed the results of the testings, about eleven of the pepsins had been tested eleven times from the same sample, and, as a rule, about three or four only of the eleven testings of the same sample agreed. In many of the pepsins the same sample, as judged by the test, gave results, in the eleven trials, differing by one hundred to three hundred per cent. between the extremes. The test gave the most uniform as well as the best results with the sample made by the reader of the paper; but even here it varied between 20 and 38, or 90 per cent. The same samples of some makers gave negative or zero results in three or four of the eleven applications of the test, while in other applications the same samples gave the figures 2 and 3. Thus the same sample by the same test showed no pepsin at one trial and three-sixteenths of an inch at another trial. It was explained, however, by the reader, but not in the paper, that when the precipitate was very small it was marked zero.

In the discussion which followed the paper, a member objected that precipitates in general, and especially those which floated at the surface of the mother liquor, varied so much in density that their volume afforded no accurate indication of quantity, and that while the author presented his test as "a qualitative and not a quantitative test," yet that the table and the wood-cut which formed a part of the paper were, in the main, and in evident intention, quantitative, and that upon their quanti-

tative value the chief discrimination between the different makers was based. The substance of the objection was that a test which could only be qualitative, at best, was used in estimating quantity, and upon its quantitative misuse, untrustworthy discriminations were made for and against individuals whose names were used. The reader of the paper then stated that he had a series of results obtained long before this paper was written, showing that the weights of these floating precipitates were in proportion to their volume.

Throughout the discussion, despite the objections, the results seemed to be generally accepted as established facts very important to be known and published, not only to let physicians know where to get good pepsin, but also to know the fraudulent practices of the makers, whose names are given.

The paper then went, in its regular order, to the Committee of Publication, where objection to its publication was made on the ground that the injurious discrimination made against respectable manufacturers was so damaging to their business interests as to render the Society liable to prosecution for damages by the publication of the names used. The author of the paper was then asked by the Committee to make the paper impersonal, by giving results without names. This he declined to do, and, although the paper was put in type, the Committee thought it best to exclude it from the Journal. Before finally excluding it, however, the Committee submitted its action to the Council of the Society, and the action was affirmed by the Council.

As the paper failed to appear in the Journal, and these circumstances became known, members who felt aggrieved by the action of the Committee and the Council brought the matter before the Society at the February meeting, and the liability of the Society to prosecution on such grounds was discussed with much difference of opinion, and with the apparent object of getting the Society to overrule the action of its Council and order the publication of the paper. The result of this discussion was the appointment of a Committee to ascertain the legal responsibilities of the Society in the premises.

It was stated that there were other considerations involved beside the liability to prosecution, and that the Council might have had other objections to the publication. But the Committee raised was restricted to the one point of legal responsibility, so that when its report is presented, any other points to be considered must come from other sources than the Committee. That there *are* other points for consideration, whether the Committee of Publication noticed them or not, appeared during the discussion. One such point was the liability of such a paper to be used as an advertisement by the maker whose pepsin appeared in it to be the only

one fit for use. In simply publishing the paper, the Society would not, of course, endorse its conclusions. But it would give it the weight and publicity of its Journal, without which it becomes far less effective. If published, it becomes a most valuable business advertisement, and might be referred to in other advertisements and circulars, by date and page of the Journal, as is customary in such cases. But if not published, such use of the Society could not be made. Such publications may be right and proper if the testing and its results are surely scientific facts accurately stated. But the Committee of Publication may have seen that the results of a test which disagreed among themselves, to an extent varying from 90 to 300 per cent., could not be scientific facts accurately stated; but could only be useful for loose advertising purposes. And the Committee may have seen that an untested test, presented as a qualitative test only, had been taken entirely out of its professed scope and character, and had been made to measure quantities which were represented numerically and set forth in a plausible looking table; and that the results thus stated could not possibly be even moderately accurate, and probably were wholly fallacious.

But the Committee of Publication could not then have known what has since very much strengthened the inference that the paper might be used for advertising purposes, namely, that it has been so used, with a slight modification of the wood-cut of the paper, on the advertising leaves of a medical journal, by the manufacturer of the lauded pepsin who read the paper before the Society, but without using the paper or its author's name, and, of course, without being able to quote from the Society's publications, as he might have done had it been published. He was present at the February meeting, and took part in the discussion. Had he then stated that he had thus used this test as his own in a published advertisement, giving the names of parties damaged by it, he would have relieved the Society from its interest in the *legal* responsibilities involved, and would have saved the raising of the Committee, because, by this publication of the test and the names, he assumed the liability to prosecution himself.

It was stated, in the discussion, that liability to prosecution could not be great, because in similar cases no prosecution had followed; and an instance was given where the Onondaga County Medical Society had, under similar circumstances, gone so far as to pass resolutions supporting one pepsin maker, and condemning others, without having been prosecuted. But it was also shown, on the other side of the question, that this action of the Onondaga County Society had been spread all over the country as an advertisement, by the makers of the lauded pepsin; and that the pepsin so lauded by *that* Society was among those now sought to

be condemned in *this* Society; and, farther, that the pepsin lauded in this paper as about four times better than the Onondaga Society's choice was among those condemned by the Onondaga Society. Such competitive and opposite action in County Medical Societies in supporting the rival claims of manufacturers, can hardly tend to improve the *materia medica* or elevate the standing of the medical profession.

During the discussion, a member spoke warmly to the point of this Society's declining to publish a scientific test given by such authorities as Stillé and Maisch, and announced that he was preparing a paper on this same subject. Another member stated that no such test was given by Stillé and Maisch, but only the process of manufacturing pepsin upon which this test was based. Although both these speakers are engaged in scientific research upon the special article on which they spoke, the scientific accuracy of one or the other *must* be doubtful.

A great deal was said about the scientific character of the paper and the discussion, and of the immunity of all such from processes of law. But both the paper and the discussion were really as loose and as unscientific as could well be. The older, more accurate and better known digestive test must be considered the standard test for pepsin, because the article is of the nature of a ferment, and is used for digestive purposes, and the quantity required to produce a given digestive effect is mainly the measure of the organic matter which protects and preserves the ferment. Although this digestive test is as easy, more accurate, done in one-fourth the time, and with no more apparatus nor manipulation, and, therefore, better and more "ready" than that now proposed—no comparison of the two was given—no check of the known test against the unknown, by which precipitation of organic matter holding the ferment as a vehicle could be judged by actual digestion.

This writer was not a member of the Council, nor of its Committee of Publication, which took action upon the paper, and can only rationally infer that they may have had some such reasons for declining to publish it. If the Council feared that it might be used as an advertisement, possibly on the cover pages of its own Journal, then its fears have been realized, for it has been so used, but on the cover pages of another journal, though without the reference to the paper which might have been made had it been published. If the Council feared that the application of an untested qualitative test to the estimation and comparison of quantities and effects was too uncertain and inaccurate to form a proper basis for charges of fraud upon a number of individuals, whose names were given, the Council's hesitation until the names should be withdrawn from the paper, and when this was refused, its refusal to publish was wise and equitable. For if processes of law do not reach the damage done by

injurious statements, made on loose and insufficient grounds, and if the law be powerless to require that damaging statements, published as facts, shall be proven, or else the damage done be compensated, this does not alter the justice and equity of the acts. Hence the moral sense and the ethics of this Society should make it avoid doubtful acts, though they be not within reach of penal law, for it may certainly be said that those in its membership

“Who have no check but human law
Are to the few restricted.”

It is so much easier to state an opinion as a fact, than to establish a fact, and it is so much easier to establish a fact than to disprove a fallacy which has been stated as a fact, that no council, nor any publishing committee, can afford the time and labor for research to discriminate. They must act by a mere exercise of judgment, and must act promptly, and therefore must make mistakes sometimes. By the cost of these mistakes the tendency is for them to learn to risk more on the conservative side, though this is often hard upon authors. So high an authority as Binz asserted in a paper published in the *Practitioner* that bromides were only sedative by the effect of the bases, and not by the bromide element, and it took a committee three months, making observations on something like 140 cases of epilepsy, to prove that this was an opinion and not a fact, and that it was true in no degree. More recently the statement that benzoate of sodium cured phthisis has taken a month or two of careful research and observation by able men to disprove the so-called fact. Meanwhile the benzoate men were making money at a great rate on the faith of the statement. And although it has been disproved, it will be some time yet before the unlucky doctors cease paying six dollars a pound for this benzoate.

The writer of this does not want to appear to be an apologist for a feeble materia medica, much less an apologist for fraud in the materia medica, and his past record may possibly show that he has not been especially timid in exposing and opposing both feebleness and fraud; but he was long ago taught the value of care and accuracy in making statements that were injurious or damaging to others, by being obliged to prove his facts, in view of their natural and legitimate consequences.

Now, as illustrated in the history of this case as given above, it was hazardous and unwise for the writer of the paper to make positive and definite statements of quantity from a qualitative test, which test, for aught that appears in the paper, may be, as it probably is, as fallacious as a test can be; for it is well known that the quantity or activity of a ferment cannot be judged of at all by the quantity of organic matter which carries it as a vehicle. As well might we undertake to judge of the

activity of a pill from its size. It was hazardous for the writer of the paper to suggest a new test as being his own, when the maker of the article which the test and the paper lauds above all others, stated to the Society that he had used it "long before the paper was written," thus leading directly to the inference that the maker of the lauded article who read the paper, and not the writer of the paper, had really suggested the test.

Therefore it was right, proper and wise for the Council of the Society to decline to admit the paper to the pages of its journal;—first, because it lauded one maker of pepsin and condemned seventeen others by name, upon loose and insufficient testimony, thus rendering the writer of the paper or the Society, one or the other, liable to prosecution for damages, and rendering the Society morally responsible for giving publicity, and therefore power to do harm, to loose and erroneous statements offered as facts to readers who might readily accept them as such.

Next, it was right and wise to decline it, because all the circumstances attending the presentation of the paper, when taken in connection with the circumstance that it was read by the maker of the pepsin whose article was stated in the paper to be from 3 to 38 times better than the others, led directly to the knowledge that, if published, it would at once become a most valuable business advertisement, the value of which would be based upon a plausible showing that might be very unsound.

If, under such circumstances, a council or a publishing committee, or an editor, is to be overruled, the utility of all such checks is very much damaged.

NOTE ON PEPSIN.

The foregoing paper was prepared for the March meeting of the Society, and therefore was written hurriedly; and for want of time could only treat the subject of the "Ready Method of Testing Pepsin" in a general way, which, however just and true, could be neither accurate nor definite. The failure of the Committee to report at that meeting, and the consequent postponement of the subject to this April meeting, gave the writer another month in which to go practically over the subject, in search of more accurate and definite results, in order that the "method" might have a fair trial by the common well-established test of actual artificial digestion, and the results of this investigation are to be here given.

Two stomachs from freshly slaughtered pigs and one pig's bladder were taken for the experiments.

The mucous membrane, with accidental portions of sub-mucous tissues, was dissected off from one stomach and was separately cut into fine pieces. The remainder of this stomach—the other stomach entire or undissected—and the bladder were also separately cut into fine pieces, and thus materials for four separate macerations were prepared. Distilled water, acidulated with hydrochloric acid in the proportion of 0.25 per cent. of dry acid, was prepared. The mucous membrane—the remainder of that stomach—and the bladder were each set to macerate separately in 1.963 grammes=30.300 grains, or about 67 fluid ounces of the acidulated water, and the entire stomach was set to macerate in double this quantity of the acidulated water. The mixtures were kept at common temperatures, say between 16° and $20^{\circ}\text{C.}=60^{\circ}$ to 68°F. , and were frequently stirred up. After 48 hours' maceration, and from that time with intervals of about 48 hours, for 21 days, 50 cc.=1 $\frac{3}{4}$ f. $\text{\textcircled{3}}$ of the liquid from each maceration was filtered off through paper, and was mixed with an equal volume of saturated solution of common salt, was well stirred and was allowed to stand covered for about 24 hours. At the end of this time there was in every trial an opalescent gelatinous precipitate of varying thickness found floating upon a clear solution. Occasionally, only, were there flocculi, in very small quantity, observed at the bottom of the vessels. These floating precipitates were filtered out through double filters, were well drained, and the filters were then closed upon their contents and subjected to light pressure between numerous folds of absorbent paper for an hour or two. The filters were then opened on a plate of glass, and when the pressure had been light the precipitate could be easily and completely detached from the damp paper by a spatula, as a semi-transparent film, almost like a thick collodion film. These films varied very sensibly in quantity when thus managed, and bore no uniform relation to the measured thickness of the layers of floating precipitates from which they came. Each film or precipitate was thoroughly rubbed up in a mortar with 2 grammes=30.86 grains of finely powdered sugar of milk, and these powders were then dried for about 4 hours at about $38^{\circ}\text{C.}=100.4^{\circ}\text{F.}$ Each powder was then put in a labeled corked vial. The weights of these powders varied considerably and bore no observable relation to the thickness of the stratum of floating precipitate.

These powders were then tried by artificial digestion. A series of cork-stopped, wide-mouth vials of a capacity of about 37.5 cc.=1 $\frac{1}{4}$ f. $\text{\textcircled{3}}$ were properly labeled for the different digestions, and a proper regulated air bath was arranged for a uniform temperature of about $38^{\circ}\text{C.}=100^{\circ}\text{F.}$

Then coagulated white of egg was prepared by boiling eggs for fifteen minutes, separating the yolk and membrane, rejecting the soft or different eggs, and pressing the white of the firmer, less watery ones, through a sieve of 30 meshes to the linear inch—mixing the product of different eggs well together, and keeping the granular mass in a stopped bottle to weigh from. Then an acidulated water was prepared containing 0.3 p. c. of H. Cl. or dry hydrochloric acid, with distilled water. The formula for all the trials was then adopted, of keeping the white of egg and acidulated water at constant quantities, and varying the proportion of the powders to be tried. That is, conditions of temperature, albumen, acid and water being kept constant, the digestive powder alone to vary, and the time required for digestion to be observed. The detailed formula was: Temp., $38^{\circ}\text{C.} = 100^{\circ}\text{F.}$; white of egg, 3.25 grammes = 50 grains; acidulated water, 15 cc. = 0.5 *f. 3*. And for all the powders prepared as described, from the two stomachs and the bladder, 0.13 grammes = 2 grains of each was taken, the whole being well shaken together before being put in the air bath, and every hour during the process. Five digestions of each of the stomach preparations were made, and two trials from the bladder preparation. These two, as was expected, proved that the powders made from the bladder precipitates were inactive. They had no digestive effect at all, though the precipitates were equal in volume with those from the stomachs.

From first to last the layers of floating precipitate, separated by the salt, were thinnest from mucous membrane, slightly thicker from the entire stomach, and nearly double this thickness from the remainder of the stomach after the mucous membrane had been separated, and from the bladder. All the layers became gradually thicker as the time of maceration increased up to the 20th day, and were at a maximum thickness when destructive decomposition or putrefaction was commencing; and, as this reaction fairly started, the thickness quickly diminished. Hence, the pepsin maker who carries his macerations nearest to the point of putrefaction will get the largest volume of precipitate by the solution of common salt. But if he permits putrefaction fairly to set in, he will get a smaller volume. But he will get a precipitate from bladders quite equal in volume to that from stomachs, though it will contain no ferment.

In the digestions of the powders prepared from these layers of precipitate, those from the mucous membrane were least effective. Those from the remainder of this stomach were more than twice as effective as those from the mucous membrane, while those from the entire stomach were much the most effective, and were very active in digestion. The effect

of those from the mucous membrane added to the effect of those from the remainder of the stomach, if taken together, would not equal the effect of those from the entire stomach. While the effect of those from the bladder was, as before stated, negative.

The time during the macerations at which the precipitates were most active did not seem to vary much. The observations upon this point were somewhat confused by unrecognized causes. From the 3d day to the 18th the activity did not vary much. But after the 18th day, as putrefaction sensibly approached, and as the volume of the layers increased, the digestive activity diminished, so that when the volume of the precipitate was at its maximum, the digestive activity was slightly less than it was three days before. There was, therefore, no discernible relation between the volume and the activity of the precipitate. But the precipitate might be very large with no digestive power at all—bladder; or, might be very large with very slight digestive power—remainder of stomach; or, might be small with slight digestive power—mucous membrane; or, might be small with active digestive power—entire stomach.

These results show that the precipitate is but a vehicle for the ferment, and may contain the true ferment in greater or less amount, or not at all. Hence the new "Ready Method of Testing" is not only useless, but will commonly mislead. That it did mislead the author of the "Ready Method" is proved as follows: The pepsins of five of the prominent makers of his list were subjected to his test, and were found to accord substantially with his tabular results. They were then subjected to the test by digestion and did not at all accord with his statements. Two samples, which were stated to be of very low value, were fully equal to his best samples, and all were fully up to the requirements of the labels. Therefore his test is entirely useless, except as an advertisement for the maker, whom it wrongly extols. A table of these digestions is given herewith.

TABLE.

Showing the Results of Testing by Artificial Digestion of the Pepsins of Five of the Most Prominent Makers for the New York Market.

The digestions were made at temperatures of about $28^{\circ}\text{C.} = 100.4^{\circ}\text{F.}$ with 3.25 grammes = 50 grains of boiled white of egg pressed through a sieve of 30 meshes to the linear inch, in 15 cc. = 0.5 *f. 3* of distilled water containing 0.3 per cent. of dry hydrochloric acid.

No. of Sample.	Test given on the label. Powder to boiled white of egg at 100° F.	Quantity taken for the test as equivalent to 3.25 grammes=50 grs. albumen.	Time required for complete solution.	Repeated digestion with smaller quantity of Pepsin.
1 . . .	10 grains to 120 grains no time or temp. given.	4.17 grs.=0.27 gram.	3 hours.	$\frac{1}{4}$ quantity in 6 hours.
2 . . .	12 to 15 times its weight no time or temp. given.	3.33 grs.=0.216 gram.	5 hours.	$\frac{1}{3}$ quantity in 7 hours.
3 . . .	10 grains to 150 to 180 grains in 4 to 6 hours.	2.8 grs.=0.181 gram.	3.5 hours.	$\frac{1}{3}$ quantity in 6 hours.
4 . . .	1 grain to 60 grains in 6 hours.	0.804 grs.=0.052 grm.	4 hours.	
5 . . .	1 grain to 60 grains in 4 to 5 hours	0.804 grs.=0.052 grm.	6 hours.	

In this connection the want of care and want of knowledge in the great mass of the medical profession may be illustrated. The last revision of the U. S. Pharmacopœia refused to admit pepsin, and consequently there is no standard of quality or strength for it, every maker adopting his own strength and stating it roughly on his label. Of the five makers here examined, only two are alike in strength. To the digestion of 100 parts of coagulated white of egg they require by their labels, and by the actual test applied, 1.6, 1.6, 5.6, 6.6 and 8.3 parts of the pepsin. It is thus seen that the prominent makers all vary very much, the pepsins of some being from three to five times weaker than others, and yet they all sell in enormous quantities at about the same price, and physicians go on and prescribe them in about the same doses.

The difference in prime cost between a pepsin that is five times more effective than another is not so great as to make very much difference in price; and as the whole stomach used in these experiments gave the equivalent of 6 avoirdupois ounces of the strongest commercial pepsin, it would appear that the profits of the manufacture are very good. Of this strongest pepsin, 1 grain of which digests 60 grains of white of egg the one stomach yielded 2.590 grains. This is equal to the digestion of 151.620 grains of white of egg. White of egg contains about 12.5 p. c. of dry albumen. Therefore, the 2.590 grains digests 18.952 grains, or more than 2.5 pounds of dry albumen, or 8 times that, or over 20 pounds of moist albumen, or more than double the capacity of the stomach of a living pig. Then again, the mucous membrane dissected off, and managed in the same way, did not give digestive results higher than about one-fifth of those given by the entire stomach. That is, if the ferment contained in the mucous membrane had acted during maceration upon the tissues of the whole stomach, it would have produced a solution of five times the power that it did as mucous membrane alone.

All this goes to show conclusively that the pepsins of the market are rather to be regarded as concentrated peptones, and not pepsin at all. They are the result of the action of the ferment upon the tissues of the stomach itself, and are simply in a condition so concentrated as to communicate their activity to other matters which may be in a condition to undergo similar change.

But such active peptones may as well be called pepsin as not, so long as they are able to induce the desired fermentation; and the only practical importance of regarding them as peptones is that if the process of making them be not a process of extraction, but one of true digestion, then it is better not to take the mucous membrane only, but take the entire stomachs instead—and not to macerate them in the cold, but at the temperature proper for digestion—and by carefully studying other conditions favorable to this particular fermentation, to obtain the most active and the most concentrated peptone that is practicable. It is altogether probable that all this has been long ago learned by the pepsin makers, but if so, the information has been kept as among their trade secrets. It is true that Mr. E. Scheffer, in his valuable paper published in the *Amer. Journal of Pharmacy* for Feb., 1872, strongly foreshadows this conclusion, but seems to regard the active peptones as mixtures of peptone and pepsin. This author, by his careful original researches, and by his frank publication of his process and results, must always be considered as the originator of this important trade interest; and his make of pepsin has always been, as it is now, among the best in the market, although not the 'strongest. A recent paper entitled: "Observations on Medicinal Pepsin and Artificial Digestion," by G. F. Dowdeswell, B.A. (cantab.), F.C.S., F.L.S., etc., in *The Practitioner* (English) for March, 1880, is important and timely, and some of the proportions used in the digestions of this paper are taken from it.

As a practical deduction from the facts observed in these experiments it seems doubtful whether physicians might not improve upon the common methods of using pepsin, when these are not successful, by putting the dose into the stomach with a little hydrochloric acid, and a little albuminous or fibrinous food half an hour or more before the meal-time, in order to produce a quantity of active peptone sufficient for the larger mass of the approaching meal. Stomachs which are deficient in the ferment are very likely to be deficient in the proper hydrochloric acid as well; and any ordinary dose of pepsin put into such a stomach after a full meal would be pretty sure to be hopelessly lost by dilution, even when other conditions might be favorable.

For the Discussion on this Paper, see the Minutes of the Society's Stated Meeting, April 20th, 1880, and of the Special Meeting, April 22d, 1880.

MEASLES NOT A TRIVIAL DISEASE.

A Report on the Present Epidemic in Brooklyn and its Treatment by the Board of Health, by J. H. RAYMOND, M.D., Sanitary Superintendent.

Since January 1, 1880, there have been 1,864 cases of measles reported to the Brooklyn Health Department; this is probably less than half the number which has actually occurred. During the same time there have been 73 deaths from the same disease, while during the entire year 1879, measles caused but 40 deaths; should the present rate of mortality continue throughout the year the record will show 240 deaths from measles for the twelve months of 1880. While measles has thus far caused 82 deaths, there have been but 65 deaths from scarlet fever.

It is a common impression that measles is a trivial disease which every child must have at some period of its life; that the younger he is the more mild the attack, and therefore the sooner he has it the better; that having once been attacked he is protected for the future; that if the disease is not contracted in the usual way, children should be taken to where the disease exists and exposed to it; that all attempts to isolate patients suffering from the affection or to prevent their return to schools or other public assemblages as soon as they are able to go are harsh and arbitrary measures, and not based on good and sufficient reasons; and finally, that as the disease can only be conveyed by the sick person himself, there can be no danger from clothing, bedding, or other material which has been in the same room with the patient or upon his body, and therefore disinfection and fumigation of these articles, and of the rooms occupied by him during his illness, are useless and unnecessary.

This is, we are satisfied, the popular opinion, and we have reason to believe that some physicians hold the same views. One of these latter, a representative of the class, writes that he thinks measles is a disease that it is rather more desirable to have than to avoid; and he does not suppose that isolation of the patient is at all advisable. From practical local observation and careful investigation of the subject, together with the experience of Brooklyn physicians obtained from their answers to a series of questions sent them by the Board of Health and appended hereto, we believe that the general impressions already referred to are entirely erroneous, and if permitted to go uncontradicted, liable to do great harm and injury, even to the degree of sacrificing human life. Let us take up these points seriatim, and endeavor to ascertain how well founded in fact these popular impressions are:

1. Is measles a trivial disease?

Aitken, writing of measles, says:

"In the year 1824 it was imported into Malta by some children belonging to the 95th regiment, and spread extensively in that island, so that many natives died."

Percival says that in one epidemic 1 person died out of every 40 who had the disease. Watson writes that in one year at the London Foundling Hospital, 1 in 10 died; at another time, 1 in 3. Aitken summarizes the mortality by saying, that—

"The aggregate of these data will give us an average of 1 death in 15. The prospects of recovery are better in the country than in the city, the records showing a greater mortality in the latter than in the former."

Nor is the danger over when the patient has recovered from the measles itself.

Ernest Hart, speaking of measles and whooping-cough, writes:

"These diseases often cause a considerable mortality among children; not directly, but indirectly. They predispose to lung diseases, especially bronchitis and pneumonia, of which the children die."

Aitken says:

"In strumous patients measles may end in the development of miliary tubercles in the lungs. * * * The cough often remains for weeks or months after desquamation is over and grows worse from the most trifling causes. It may depend on simple bronchial catarrh or on severe disease of the lungs. The nature of that disease, however, is not always tubercle, but more often a caseous transformation and disintegration of the products of lobular pneumonia with caseous degeneration of the bronchial glands—one of the most common complications of measles. Croup sometimes supervenes and cuts off young patients. It tends to be of the asthenic type and is not unfrequently preceded by diphtheritic inflammation of the fauces which gradually passes down to the larynx."

The physicians of Brooklyn report 54 cases of measles which have been followed by diphtheria, some of them fatal from this cause:

"Diarrhoea is another danger to be encountered." * * Aitken writes: "If suffered to continue the consequences may be fatal."

"Catarrhal ophthalmia, otorrhoea, swelling of lymphatic glands, if the constitution be strumous, must also be watched for, and if possible prevented."

2. Is measles a disease which attacks a person but once?

On this subject Aitken says that as a general principle the patient is exempt from liability to a second attack, but he also adds that Burserius, Robedieu, Home, Baillie, Rayer and Holland, have all seen instances of a second attack of measles in the same individual.

Ernest Hart writes: "Second attacks are not very uncommon, and third attacks are not unknown."

Austin Flint, Sr., says: "Well authenticated cases in which the disease (measles) has occurred three or even four times have been reported."

The experience of the Brooklyn physicians is very large, and their evidence in this matter, obtained from the circulars before referred to, is

very strong. They report that second attacks have occurred *under their own observation* in 210 instances, and third attacks in 7 instances. This shows at once the folly of exposing children to the disease that they may "get it and have it over with," for in the first place there is a possibility of the disease itself proving fatal, or if the children recover from measles they may die from its sequelae, croup, or diphtheria, or diarrhœa, and if they pass through all these dangers they may still have miliary tuberculosis, or some other pulmonary disease, and die from that; but granting that complete recovery takes place, they are not protected from a second attack of the disease, or even from a third. But it is said that if it does occur a second time it is in a very mild form. This brings us to the third question:

3. Are the recurrences of measles modified by the previous attacks?

One hundred and thirty Brooklyn physicians report that the second attacks have not in any degree been milder than the first, but have been unmodified by the previous ones; 36 report that the second attacks have been more severe than the first, and only 30 report the disease as modified in its recurrence. One physician reports a second attack after an interval of 3 years as ending in death.

4. Is measles conveyed by fomites?

This is in a sanitary point of view a most important question to decide. If it cannot be so conveyed then there is no danger from the clothing of the patient, nor from the clothing of those who attend him in his sickness; nor can members of the family or those living in the same dwelling carry the disease to others; nor is there any necessity for disinfection or fumigation of these things, after recovery; but if, on the contrary, the disease is propagated by fomites, all these precautions must be taken if we would prevent the spread of the disease; in other words, the same isolation, disinfection and fumigation should be practiced for measles as in small-pox or scarlet fever.

On this point Niemeyer says:

"From some very striking observations of Panum it has been proved that this contagion in the atmosphere can, without losing its activity, be carried for miles by the body and clothes of healthy persons who have been near a patient, and who are not themselves attacked by the disease. * * The probability of infection during the prodromal stage is supported by the wonderful spread of measles through schools. Great care is usually taken to keep out of the school any children who have not gotten through the desquamative stage, as well as those having any suspicious exanthem; but children with catarrh and cough are allowed to sit on the seat with well children."

Aitken's testimony to the same effect is very striking:

"This disease is also propagated by fomites. The strictest demonstration of this fact is that the disease has been communicated by direct application of substances impregnated with the virus in the attempts to inoculate the disease; it is also proved by

the fact that children's clothes, sent home in boxes from schools where the disease has raged, communicate the disease, and also by the same circumstance resulting when susceptible children have lain in the same bed or in the same room shortly after it has been occupied by patients suffering from the disease."

Hart, writing of measles and whooping-cough, says:

"Like the other diseases of the same class, they are eminently communicable by means of infected air and clothing," and he adds: "in the case of measles by means of the contagious discharges."

This opinion is very generally held by the best authorities. Charles Cameron writes of measles:

"It is highly contagious, and the measures necessary to prevent the spreading of it are similar to those to be employed in the case of small-pox."

Eighty Brooklyn physicians believe it to be spread by fomites; 36 do not, while 20 are undecided. One physician writes:

"I am confident that I conveyed the disease by my clothing to one of my children. I called to see a case of measles a couple of blocks from my house; came immediately home, and thoughtlessly picked up my little girl and placed her on my lap before removing my overcoat. I dropped her in a few minutes with the remark that I had just been to a case of measles. In about eleven or twelve days the child was taken with measles. She had not been out of the house for a couple of months. There was no measles in the immediate neighborhood. She had not been in contact with any one having it, and I know of no other way she could have contracted the disease. Dr. C. informs me that he conveyed it to his child in the same manner."

5. Is measles highly contagious?

Cameron says: "It is highly contagious." Hart speaks of it in the same terms. Aitken writes:

"Like scarlatina, measles is thus eminently communicable; and, in like manner, no susceptible person can remain in the same room, or even in the same house, with an infected person without hazard of taking the disease. The infecting distance of this poison (that of measles) must be considerable; indeed it is often very difficult to isolate the disease in public schools, or other large establishments where it sometimes appears."

Bristow declares that:

"Measles is one of the most virulently contagious of diseases. * * The presence of a case of measles amongst a number of unprotected persons will, as a rule, induce a more certain and wide-spread outbreak of disease than either of the other exanthems would do under similar circumstances. Its contagiousness is fully developed at a very early stage, being at its height on the second, if not on the first, day of invasion, and consequently before the specific nature of the attack is revealed. Hence the great difficulty, if not impossibility, of effectually preventing its spread in households and in schools."

Frederick Roberts writes:

"Measles is decidedly infectious, especially when the eruption is out; and its contagium passes off abundantly in the exhalations of a patient, the air around being thus contaminated. It is also conveyed by fomites. Children have undoubtedly taken the

disease from sleeping in a bed or room formerly occupied by a patient suffering from measles."

Austin Flint, Sr., says:

"Rubeola, like scarlatina or variola, is a communicable disease. The infectious miasm is not only received by those brought into close proximity to persons affected with the disease, but it may be transported to a distance by means of fomites. Persons contract the disease from the miasm adherent to the clothes of those who have recently visited rubeolous patients. Physicians may in this way diffuse the disease."***

One hundred and thirty-nine Brooklyn physicians regard it as highly contagious; 1 as moderately contagious, while 15 report it as not highly contagious; 60 of these regard it as more contagious than scarlet fever, 46 as less contagious, and 45 as equally contagious.

In speaking of contagious diseases, measles included, Hart says:

"All these diseases are propagated more than anywhere else at schools; and during epidemics the greatest precaution ought to be taken in sending children to schools, especially as there is every probability that some of these diseases, if not all of them, are contagious during the period of incubation."

In view of the facts that measles is at the present time epidemic in Brooklyn; that it has already in 1880, as stated above, caused 73 deaths, while during the whole of 1879 there were but 40 deaths; that it is "one of the most virulently contagious of diseases" (Bristow); that "its contagiousness is fully developed at a very early stage of the disease * * before the specific nature of the attack is revealed" (Bristow); that it is conveyed by fomites; that "persons contract the disease from the miasm adherent to the clothes of those who have recently visited rubeolous patients" (Flint); or "from clothes sent home in boxes from schools where the disease has raged" (Aitken); "that no person can remain in the same room, or even in the same house, with an infected person, without hazard of taking the disease" (Aitken); that one attack does not render a person non-susceptible; "that the measures necessary to prevent the spreading of it are similar to those to be employed in the case of small-pox" (Cameron); in view of all these facts, the Board of Health, under the Code of Sanitary Ordinances, directs the exclusion from school of all children living in a house where measles exists, and prohibits their return until the case is well, and the premises fumigated with sulphur.

The following is a series of questions sent to the physicians of Brooklyn by the Board of Health, and an analysis of 155 responses received thereto:

Is measles, in your opinion, highly contagious?

139 physicians answer, Yes. 15 answer, No. 1 answers, moderately so.

Is it, in your opinion, more or less contagious than scarlet fever?

60 answer, More. 46, Less. 45, Equally contagious.

Is it, in your opinion conveyed by fomites?

88 answer, Yes. 36, No. 20 Undecided.

Is measles, at the present time, in your practice, unusually malignant?

14 answer, Yes. 124, No. 12, Severe.

How many cases have you had in which diphtheria has supervened upon measles?

54 such cases are reported.

In how many instances, *under your own observation*, has measles attacked the same person more than once? or more than twice? and at what intervals?

210 second attacks are reported, and 7 third attacks. The intervals vary between 2 weeks and twenty-eight years; the usual interval being about 3 years.

Have these recurrences been severe, or have the prior attacks apparently modified them?

36 answer, Recurrences more severe than the first attacks. 130, Recurrences have not been modified. 30, Recurrences have been modified.

COMPLICATIONS IN 59 DEATHS REGISTERED AS MEASLES.

Nervous.		Pulmonary.		Intestinal.	
Meningitis and } Hydrocephalus } ...	6	Congestion of the lungs	3	Dysentery.....	2
Convulsions	13	Pulmonary Apoplexy..	1		
Congest'n of the brain	2	Bronchitis.....	9		
		Pneumonia	18		
		Laryngitis.....	5		
Total 59.....	21		36		2

In addition to the above, 14 deaths were reported as due to measles without any complications.

The following deaths not being caused directly by either measles or its complications, were not registered as due to measles, and do not appear in the above table, although they were certified by the attending physicians as having measles as an intercurrent affection:

Meningitis, 1; convulsions, 4; pneumonia, 2; bronchitis, 2; diphtheria, 4; pulmonary congestion, 1; dysentery, 1; scarlet fever, 2—total, 17.

ACTION OF THE BOARD OF HEALTH.

Measles being at the present time so prevalent in the City of Brooklyn, and its mortality since January 1st, 1880, so great, the Board of Health has included this disease in the same category with scarlet fever and diphtheria, and requires the following action:

1. Reports to be made to the Health office by physicians, of all cases coming under their care.

2. The exclusion of the sick and of others residing in the same house, from the schools of the city, both public and private, until a permit for their return is obtained from the Board of Health.

3. These permits to be given when the patient is no longer in condition to spread the disease, and when the rooms, clothing, and other infected materials have been properly fumigated.

4. The fumigation prescribed by the Board of Health is by the burning, for five hours, of sulphur, one pound to each thousand cubic feet of space to be fumigated, the apartment being tightly closed.

5. Certificates of physicians that these requirements have been fulfilled will be sufficient evidence, and on their presentation to a sanitary inspector or at the office of the Board of Health, the school permit will be at once issued.

THE MEDICAL SOCIETY OF THE COUNTY OF
KINGS.

EXTRACT FROM THE MINUTES OF THE REGULAR STATED MEETING, FEB. 17TH,
1880.

DR. WM. WALLACE, referring to a paper read before this Society some months ago by Dr. Merritt, upon "A Ready Method of Testing Pepsin," inquired whether it had been given up or merely postponed.

DR. HUTCHINS replied that by direction of the Council it had been indefinitely postponed.

DR. L. C. GRAY asked for what reason it had been so postponed?

DR. HUTCHINS said the facts were about these: Dr. Merritt read a paper whose title was "A ready Method of Testing Pepsin." The Publishing Committee were spoken to by members of the Society to the effect that the form in which that paper was put, with the additions made to the subject matter, in addition to the title, was such, that if published under the auspices of the Society, it would lay the Council, who were directly in charge of THE PROCEEDINGS, under a risk of prosecution by business firms whose reputation would be affected by the assertions of the Society in its publications

as to the inefficacy of these particular drugs. The Publication Committee then sought out the author of the paper, told him of their convictions, and asked him if it could be put in such a shape as to be impersonal. He did not feel as if he wanted any change made in the paper. The matter was then, of course, referred at once to the Council, whose agent the Publication Committee is, and the Council endorsed the action of the Committee in withholding the publication.

DR. GRAY had been surprised, during the last week, to hear considerable discussion in regard to the action which has been taken in regard to this paper—a discussion which was in no way inimical to any gentlemen upon the Council, or personal in any manner to Dr. Hutchins, whose character had been so handsomely spoken of at the meeting. But there seemed to be a feeling that there is behind all this a principle that if worthless preparations were put upon the market, and the worthlessness of the same was exposed by some competent physician, medical societies should be free to publish the strictures—otherwise physicians would be at the mercy of those individuals. It seemed to him that it ought to be clearly ascertained whether any liability attaches to such publications. If the Society is not liable, and the paper is not published, such action is simply truckling to manufacturers of worthless drugs when such publication is not made.

The Medical Society of the County of Onondaga, in this State, had published a paper giving hints in regard to the value of various preparations of pepsin. If no one had been sued for that, he hardly thought this Society would be liable, and it seemed absurd to consider that suit would be brought by people interested. Under the circumstances he thought it proper that the matter should be looked into very much more closely than it has been, and reports made to the Society. It certainly seemed to him that physicians have a right to know whether any preparation of pepsin employed is better than any other preparation on the market. To say that such information can only be communicated to one, and this orally, in a whisper or sub rosa, seems something monstrous! He spoke upon the subject rather reluctantly because of the fear of being misinterpreted as meaning something personal to the Council or gentlemen composing it. Such was not his intention in any way. Therefore, if a motion were in order, he would offer the following:

Resolved, That a Committee of three be appointed by the President to inquire into the legal aspects of this matter, and report at the next meeting of the Society.

DR. E. R. SQUIBB arose for the purpose of calling attention to the fact that this well-known principle which Dr. Gray had enunciated might be coupled with another principle; and that is that when a paper read is personally directed to a comparison of all preparations of a certain article on the market, as, for instance, pepsin, and of all the prominent makers of pepsin, it is not simply a declaration about one brand of pepsin, but it affects all; it declares that all are worthless except one, which is possessed of superior virtues. There is where the legal aspect of the case will be found to come in. Then these endorsements are liable to be used as advertisements; and if the gates are opened thus by this one paper a precedent will be established for the reading of similar papers for which he thought the Society would be sorry. Still, he was not here as an advocate of the muzzling process. But the paper was accompanied by a cut having makers names attached, showing the different degrees, purity of excellence of the article manufactured by them and the superior excellence of one particular brand. It is for this reason, among others, that he objected to a reversal of the decision of the Council. Should the names of manufacturers be omitted, there would be no liability—the whole thing would then go upon its merits.

It is well known further that the freshness of pepsin renders it very variable, or at

least somewhat so. Very old preparations will not respond as favorably to the test as more recent specimens; and there are a great many circumstances which enter into the calculations as essential factors. In these it is not fair to draw such comparison.

Now, for this Society to give its adherence to one particular mode of testing pepsin as being its own test, and basing its decision on the authority of one single paper is what he would find fault with.

He fully concurred in the action of the Council, although he was willing that a committee should examine into the subject; he thought that if the committee would examine into the matter closely—which of course they will—they will fully agree with the Council.

DR. GRAY did not wish to be understood as desiring that this paper should be published *volens volens*. If the Society is clearly liable, and if it is wrong to publish that paper, let it be so understood; that will be for this committee to determine. He thought Dr. Squibb's reasoning fallacious in several particulars. If his reasoning were carried to its logical extreme, no paper can be read which reflects upon any man's reputation, as for instance, a difference of opinion in the matter of diagnosis. But whether the reflection is as against a manufacturer of pepsin or a physician regarding his diagnosis, there is no difference in principle. The only difference is, if there is any, that a medical man will not be liable to sue, whereas the manufacturer will. He never heard of a doctor suing.

In reply to another point mentioned by Dr. Squibb, he held in his hand a circular, being an extract from the proceedings of the Onondaga County Medical Society as reported in the "Standard," June, 13, '77. It gives the result of an analysis of some fifteen different samples of pepsin from as many different makers, and also a resolution adopted by the Society as follows:

"WHEREAS, In view of the necessity that the profession should have pure drugs and medicines in order to do their patients and themselves justice, and in view of the tests that have been made by James S. Plumb, druggist, of different samples of pepsin offered us for use, and the results of that test have been made so plain; therefore,

"Resolved, That we, the members of the Onondaga County Medical Society, do recommend the article of pepsin, manufactured by Messrs. Smith & Pitkin, as preferable to all others for medicinal purposes."

That Society was not sued, and nobody connected with it was sued. It seemed to him if the Onondaga County Medical Society could afford to do that, the wealthy Society of the County of Kings can still better afford to do it!

DR. SQUIBB claimed that the Onondaga County Society could not afford to do it, because that very statement has been used as an advertisement for the very pepsin recommended, and to the disadvantage of the others. (The injustice of the resolutions quoted may be shown when it is known that the pepsin is declared in the paper to be even worse than the one under criticism.)

DR. KRETZSCHMAR was generally very much interested in the matter under discussion, inasmuch as he is on the list to read a paper on pepsin at the April meeting, in which tests are mentioned.

DR. J. S. WIGHT thought the true vendor of drugs, whether manufacturers or not, stood in the same position as the vendor of victuals: he warrants the article. The man who makes and sells bad drugs is the warrantor, and he is liable, as an individual. He is liable, because he has broken the bond of his warrantee.

DR. SANFORD was surprised that any question should be raised as to the liability of individuals of a society for strictly scientific discussions. Money damages must be shown before a plaintiff can have any standing in a court of law. He thought scientific discussions did not come under the rule as for slander.

DR. WESTBROOK was impressed with the importance of the principle as to whether a person reading a paper before a society is, or is not, entitled to have the same published in THE PROCEEDINGS. Papers read here are scientific papers, and stand in that light. One member reading a paper has the same right to have his paper published as another.

DR. WALKER asked whether the objectionable matter occurred in the paper or in the discussion.

DR. HUTCHINS, in answer, referred to the matter of the paper and the method of presenting it—being illustrated by experimenting with the various pepsins made, and a free use of manufacturers' names on test-tubes and charts. The title of the paper was "A Ready Method for Testing Pepsin." Thus an opportunity was given to make the action of the Society the basis of an advertisement. The difficulty was in the contents of the paper.

DR. HAWLEY, in 1874, published a short article referring to a particular brand of pepsin, showing that it did not fulfill one promise made, that it was a scientific conundrum, and that it was a physiological absurdity. He could not have said much more against any pepsin than he did on this occasion. He mentioned the name of the manufacturing firm in full. It occurred to him afterwards that he was possibly going too far, and he therefore went to a very highly reputable lawyer, gave him the paper, and asked him if anything could be done with him. "Do you mean to ask if it is actionable?" said he. Being informed that that was the intent of the inquiry, he looked the paper through and through; then with a sneer upon his face and a look of savage scorn he answered, "No, sir; scientific investigations are not actionable!"

That was the opinion of the lawyer; and the doctor took his scornful look as a reprimand.

DR. MERRITT said the title of his paper, as given the President, was "A Ready Method of Testing Pepsin with Experiments."

He also referred to the various tests of pepsin.

The question was then called for.

DR. SQUIBB asked whether Dr. Gray proposed to limit this resolution to the legal aspects of the question only or to include all the principles involved in the publication of such papers?

DR. GRAY would not make it so broad as that, for in that case it would be difficult to come to a definite conclusion.

THE SECRETARY moved that further discussion of the question pass to some further meeting. The motion was lost.

The resolution of Dr. Gray was then adopted, and it was so ordered.

THE CHAIR appointed Drs. Gray, Sanford and Wyckoff as such Committee, and the Society adjourned.

MINUTES OF THE MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A REGULAR STATED MEETING was held on the evening of Tuesday, April 20th, 1880, at No. 398 Fulton Street, Brooklyn, at 8:10 o'clock; the President, CHARLES JEWETT, M.D., in the chair.

About sixty-five members of the Society were present.

The MINUTES of the previous meeting, as published in the April PROCEEDINGS, were read by J. H. Hunt, M.D., Assistant Secretary (who, in the absence of the Secretary, Dr. Wyckoff, officiated at this meeting), and approved.

The following propositions for membership were made:

By Dr. Russell: Thomas Gallagher, M.D., Bellevue Hospital Medical College, 1879, Manhattan Avenue and Java Street; by Dr. Shaw: John A. Arnold, M.D., Kings County Asylum; by Dr. Rushmore: F. H. Miller, M.D., East New York.

The COUNCIL reported the acceptance to membership of the following duly nominated candidates: C. L. McCann, M.D., University of City of New York, 1880; C. H. Johnson, M.D., University of City of New York, 1880; W. E. Conway, M.D., College of Physicians and Surgeons, New York, 1880.

The CHAIR announced the following Committees for the year 1880:

Committee on Hygiene: Drs. J. Walker, B. Edson and N. B. Sizer. Committee on Registration: Drs. W. E. Griffiths, A. S. Clarke, A. J. Jenkins, G. W. Russell, N. Matson, F. W. Rockwell, R. M. Buell.

The Society then listened to a paper, by Dr. L. C. GRAY, entitled: "A Case of Paralysis of all Extremities, due to a Myelitis of the Anterior Cornua." He illustrated his paper by diagrams and a patient.

Remarks were made in debate by Drs. Shaw and Gray.

The second paper was read by Dr. P. H. KRETZSCHMAR, entitled "Notes on Pepsin."

The following named gentlemen took part in the debate: Drs. Wallace, Hawley, Merritt, Kretzschmar, Sanford, Chapman, Walker, Read, Gray and Jewett.

At this point of the proceedings a short discussion arose as to whether it would be advisable for Dr. Squibb to read his paper, announced on the card, because of the lateness of the hour, as it was then ten o'clock.

A suggestion was made that it might be read by title and published in THE PROCEEDINGS.

DR. SQUIBB said that on account of the lateness of the hour, and if the Society was willing, he would forego the reading of this paper and publish it himself. The Society would then have an opportunity of reading it.

A motion, however, prevailed that the paper be read at this meeting.

DR. SQUIBB then read the third paper of the evening, entitled: "The Relations of the Medical Profession to the Trade Interests of the Materia Medica."

After the reading of this paper an animated discussion arose, in which

DR. GRAY remarked that as Dr. Squibb's paper, or, at least, the better part of it, refers to the subject upon which the Committee commonly known as the Law Committee was appointed, it therefore appeared proper that he should rise to note some of the deductions and some of the principles laid down. But in order that he may be perfectly fair and clear as to the question, he would like to state what he understands to be Dr. Squibb's propositions. If he was wrong in his impressions he desired to be corrected. He understood Dr. Squibb to say that this Society, by having a paper read before it, endorses that paper.

DR. SQUIBB said the impression was wrong. The Society does not endorse, but simply gives publicity to it.

DR. GRAY, in the second place, understood Dr. Squibb to say that, inasmuch as papers may be read before this Society which may be made use of by manufacturers by way of advertisements, therefore it is wise that the Editors or Council should suppress such papers.

DR. SQUIBB replied that that was not exactly what is said in the paper. He said that when names are used the Council should suppress them, as the Journal of the Society should be impersonal.

DR. GRAY understood Dr. Squibb to say further, that the Editor of the Journal is absolute; that the Society ought not to interfere with the Editor in his functions.

DR. SQUIBB replied that that was also a wrong impression. The Editor of the Journal is not alluded to; it is the Publication Committee, and the Publication Committee is amenable to the Council.

DR. GRAY asked if he understood Dr. Squibb to say that it was unwise to interfere with the editor of the Journal?

DR. SQUIBB replied: No; he was sorry that his paper was not more clear. The ground taken in the paper, as near as he could express it, is, that if a paper is submitted to the Publication Committee for the purpose of scrutinizing whether that paper is a proper one to be published in the Journal of the Society or not, it is the duty of that Committee to do so. If every paper that is read before the Society is to be published, then there is no use for the Publication Committee or Council; that is another thing. The tendency of the discussion indicated that position.

DR. GRAY: Well, the conclusion is that it is unwise to interfere with the Editor?

DR. SQUIBB: All right.

DR. GRAY: Then it seems that Dr. Squibb is not understood, after all.

In regard to the first point, that it is unwise for us to publish papers which purport to contain experiments made in regard to the value of pepsin made by certain manufacturers, because that paper and its statements may be made use of by manufacturers, in order to advertise their wares.

That is essentially the old principle which has been given up in this country, but still retained in Europe, namely, a press law.

It is the stifling of discussion. If a man reads a paper before this Society in good faith, this Society is not to be held responsible if what is said in the paper is made use of for advertising purposes by manufacturers. That (the improper use of paper by manufacturers), it seemed to him, is a thing with which the Society had nothing to do. If that is the case it will be necessary to refuse other papers, for fear mischief may follow. All these different experiments bring out different results. In the paper read before the Onondaga County Medical Society, Smith & Pitkin's pepsin was ahead; in that read before this Society, (Merritt's), Hawley's was ahead, and in the paper read on this occasion (Kretschmar's), Scheffer's wins the belt! The result of it all is that a great deal has been learned about pepsin—more than was known by the Society probably before; and it seemed to him that these facts alone are a complete vindication of the wisdom that would have been found in publishing Dr. Merritt's paper, inasmuch as Dr. Squibb admits that this Society is not responsible for the statements made therein.

There was another argument which might be advanced against the principle of suppressing Dr. Merritt's paper, which has been reflected upon as being not strictly scientific as to its experiments, and that is, if it ought not to be published, and their paper admittedly scientifically accurate, ought not to be published. In other words the Edi-

tor and Council assume the right not to publish this paper because the experiments detailed therein are inaccurate. Dr. Kretzschmar comes along fraught with experiments which are admitted to be scientifically correct, and which in the main confirm Dr. Merritt's paper, and prove that the Council are wrong. If Dr. Merritt's paper had not been brought into public notice, it is quite evident that a large mass of valuable material would have been lost; and this is a strong argument in favor of its publication.

DR. SANFORD remarked that, were it not that he had been placed on the Committee known as the Law Committee, and to a certain extent this subject has therefore been considered by that Committee, and also to the fact that he was one of those who had participated in a discussion upon this same question on a former occasion, and he was, therefore, one of those to whom Dr. Squibb had given the reputation of being "unscientific," he would not venture to say a word now. The points which Dr. Gray brought forward seemed to him to be conclusive. It shows that the former paper was not suppressed because names were mentioned, for if that were true, the paper read to-night should be suppressed. Should we allow such a discussion as has been held to-night, requiring a large amount of time and labor for its elaboration and presentation, to be suppressed from THE PROCEEDINGS of a Society like this simply because the names of certain men are affected thereby? Can it be possible that we are to grind ourselves down to a position that when a man reads a paper here and mentions a drug or a surgical instrument, that the name of that drug or instrument shall be suppressed, and everything said about them? Every name mentioned shall be blotted out, for fear that somebody may find it in THE PROCEEDINGS and make use of the record for his trade's interest? If so, the report of the meeting to-night must be suppressed on the same principle. Is not that carrying the thing to an absurd extreme?

In regard to the paper of Dr. Squibb, he must say that that part bearing upon the exclusion of the transactions is personal. Inasmuch as he followed the reading carefully, and inasmuch, also, as he said it was hastily written, he felt it would be wrong if his interpretation of what occurred at the two meetings at which the subject had been discussed should go out as the accepted version. If he recollected correctly, there was but little difference of opinion in the discussion of the paper, and Dr. Squibb stood alone on the other side. He would move, if it was in order, that that part of Dr. Squibb's paper having reference to what occurred, which he described, should not be allowed to go upon the record of the Society through its PROCEEDINGS. Carried.

DR. HAWLEY arose to a question of privilege.

DR. SQUIBB had insisted that all papers read before the Society should be *impersonal*, and had intimated that the paper read by him (Dr. Hawley) was read for advertising purposes.

That was not so, and it was a cruel injustice to assert to the contrary. He protested against it, that his name should be connected so unworthily with any part of this business, and which makes the paper of Dr. Squibb fully as personal as any other.

DR. MERRITT desired to state that the paper about which there has been so much discussion, was ready for presentation at the October meeting of this Society. It was, by arrangement, to be called for at that meeting. Through the failure of memory on the part of the Chairman, it was not called for. He was here, at the November meeting, ready to read the paper himself, and the President stated to him that it would be accepted as having been read by title, and requested to have the paper sent to the Committee, which was done, and he supposed it would be published in THE PROCEEDINGS. When the Journal came out, it was not published. He had no idea that he would be called upon to read the paper, until the morning of the day upon which

the December meeting occurred, when he received a note from a member of the Committee, stating that he enclosed a copy of the proof to read the paper from. He was engaged that night, and he endeavored to find some one to read it for him. It was getting late, and he could not find any one to read it. Knowing that Dr. Hawley had made similar experiments to those made by himself, independently of his own, and being familiar with the manipulations necessary, he went to him, as a last resort, and begged him to read the paper for him, and he consented to do so as a personal favor, *not* as an advertisement for Dr. Hawley at all.

He also wished to state, in regard to the publication of names, that he received a note from the Publication Committee, asking if they could publish the paper without names. He submitted the matter to five members of the Society, who said they would not consent to it. Therefore he refused to omit the names from the paper.

DR. WALKER thereupon moved that Dr. Merritt's paper be published in the next number of THE PROCEEDINGS.

DR. SQUIBB, in seconding the motion, disclaimed any personality in the matter against any member of the Society. He had no personal objection to the publication of the paper if the result of the two months' labor which he had given, in order to show the falsity of those tests, shall be published with it. If any member who brings a paper here expects any active member to spend two months in refuting it, it should be so understood. He found that he was very foolish in spending his time in disproving the paper. It is not worth it; it is wasted time to spend all the spare moments that he could get in this object. If we are to accept papers here without having any counter check, let us know it and publish anything that comes before the Society.

He had no objection to the publication of this paper, provided all that Dr. Kretzschmar has said, and all that he has said about the fallacy of the test be published with it. It had been decided that a part of his paper is not to be published. Why except that portion? Why accept any of it? He was willing to withdraw the whole of the paper, only he should like the experiments which he had made to go for what they are worth in proving the fallacy of this test.

Dr. Walker's motion was then agreed to.

DR. SHERWELL moved that the resolution not to publish the history of the discussion as related in Dr. Squibb's paper be reconsidered. He considered it an injustice that Dr. Merritt's remarks be published and Dr. Squibb's paper suppressed; he therefore moved to reconsider Dr. Sanford's motion.

DR. SQUIBB said he did not desire any inaccuracies. If anybody will correct them, then he would at once accept any action which the Society might see fit to take. He did not want to see the Society used for base purposes, neither did he desire to publish inaccuracies.

DR. WALKER moved as an amendment, provided Dr. Sherwell accepts, that the matter be left to Dr. Squibb to look over and revise.

DR. HUTCHINS here arose to a question of privilege, and was beginning to address the Society in relation to certain matters relating to the library, when several members raised the point of order that there was a motion before the house.

THE CHAIR ruled—that the subject before the house was, that Dr. Squibb's paper be published with certain exceptions. There were also several amendments. One to refer the paper to Dr. Squibb for the purpose of proving it; one that the paper be referred to a committee of three for the same purpose; and still a third that all papers be referred to a committee of three.

A motion to lay the whole matter on the table was offered but did not prevail.

Each amendment was then put and lost in turn.

The resolution of Dr. Sherwell was here voted upon and declared lost.

A division being called for, it was found that there were ayes 17, nays 11; and it was declared carried.

Dr. Sanford's motion was then put to vote and lost.

DR. HUTCHINS here addressed the Society in relation to the loss of certain valuable journals and books, and the difficulty of exercising an adequate oversight as to library matters, since there was no custodian and the hall and library room were more or less open to the public. For this reason no one in particular could be blamed.

DR. HAWLEY felt that he had not been properly vindicated. He therefore moved, and it was seconded, that all language in that paper (Dr. Squibb's) relating to a manufacturer whose pepsin was lauded, who read the paper, together with other disreputable things, be stricken out. Carried.

DR. GRAY moved that the committee appointed to examine into the legal aspect of the Society publications, which had been appointed at the February meeting, inasmuch as there seemed to be no further occasion for its services, be dismissed. It was so ordered.

DR. BURGE moved that the Council be authorized to offer the sum of \$25 for the conviction of any person who shall be discovered to have taken books, pamphlets, or journals from this library.

This motion was not seconded and therefore not acted upon.

Adjourned.

DEBATE ON NOTES ON PEPSIN.

DR. WM. WALLACE asked whether some of the preparations used kept better than others.

DR. KRETZSCHMAR replied that he used samples kept in first-class drug stores, and supposed, of course, they were all about the same age. He knew the English preparations were rather fresh because they were laid in only a short time ago.

DR. JAS. S. HAWLEY would like to call attention to one or two points; and first he would notice the experience quoted in the paper of Dr. Chapman, and he was very well aware that Dr. Chapman and himself were at variance upon the subject of Pepsin; nevertheless, when he claims that the coagulated caseine found in the dejections of infants is increased by the administration of Pepsin, he had only to say that it has been an observation of himself (Dr. H.) made repeatedly to ascertain whether their dejections would disappear under the administration of Pepsin, and he had found them invariably to do so. Which statement is to be credited, others may judge.

Secondly, with regard to alcohol in preparations of Pepsin.

He could fully agree with Dr. Kretzschmar as to the virtues of elixir, but he believed there was a general misapprehension as to the effect of alcohol in liquid preparations of Pepsin. He supposed every man in the room knew that the distinguished physiologists and physiological chemists who have demonstrated the existence of Pepsin, have demonstrated also its qualities and offices in the art of digestion, and without their labors he would probably know but little upon this subject. He supposed that gentlemen also knew that these same gentlemen have used alcohol as a precipitant by which they obtain their Pepsin, which is a proof that alcohol does not destroy the digestive power. The only question, therefore, is whether its presence counteracted its digestive effect. In the test-tube the presence of alcohol does prevent digestive action; but it must not be forgotten that in a mixture containing a certain percentage of alcohol, a large amount of Pepsin may be held in solution. Of course, it may be urged, that this is true up to the point of saturation. But the stomach is a dialyser

and alcohol remains in the stomach but a short time, the Pepsin, being non-crystallized, of necessity remains behind. Here, he believed, was the true explanation of the effect of Pepsin wine.

There was another point, and that is with regard to the weight of the precipitates produced by Dr. Merritt's method of testing.

We must accept Dr. Kretzschmar's statement as the fact; but it is at variance with observations made by himself (Dr. H.), before Dr. Merritt wrote his paper. He had in his possession a series of desiccated precipitates. The precipitates were made and desiccated by humidity and taken to a neighboring druggist, where he knew he had an accurate scale of decimal weights, and the weights were made by the druggist and marked. Dr. Hawley had the curiosity to preserve them, and he desired to say that generally the weight corresponded with the volume of the precipitate, and that the series of experiments were made without any intention of using them for advertising purposes.

DR. MERRITT would like to ask Dr. Kretzschmar whether he disposed of the chloride of sodium that was entangled in the precipitates. That, of course, would have to be disposed of before an accurate weight of the precipitate could be determined. The quality of the Pepsin may have a very decided influence upon the amount of the chloride of sodium which is entangled. A precipitate which is light and gelatinous and very dense—a heavy precipitate to the eye, but not to the scales, in Dr. Kretzschmar's experience, would probably not entangle so much chloride of sodium as one more leathery and dense. Dr. Kretzschmar called attention to the uniformity of results, or similarity of results of his own experiments and those made by foreign observers; but he does not call attention to the fact that the results of his digestive tests and the results of volumetric estimate of Pepsin in his (Dr. Merritt's) test are about the same—they correspond quite as closely; and he had made a number of digestive tests.

DR. KRETZSCHMAR most willingly acknowledged that Dr. Merritt's tests and his own most generally agreed, but he did not consider it quite as accurate. Now, McKesson & Robbins' Pepsin is a 30 per cent. Pepsin, and Dr. Merritt wrote it down as (—)per cent. That was his only intention in speaking of or mentioning the tests of Dr. Merritt—it is not as accurate, and if a test is not accurate it don't amount to very much.

In regard to the chloride of sodium: When salt is used, the precipitates may not come up, and they may also at times appear larger than they really are. It is said that they do not agree with the volume test. Some of those which showed no Pepsin by Dr. Merritt's test, afterwards must have precipitated something, because they weighed one-half a grain, and *nothing* cannot weigh one-half a grain!

As to Dr. Hawley's statement about alcohol: His (Dr. H.'s) last statement does not agree with his first. He further desired to state that what he did say was that strong alcohol interferes with the precipitates of Pepsin, and Pepsin wine is a very fair method of administering Pepsin.

DR. HAWLEY said, as to the matter of alcohol, he simply quoted the practice of physiological chemists to show that alcohol would not destroy the digestive power of Pepsin. Pepsin must be dissolved before it is added to the wine to make it a successful operation. A weak wine will not make a precipitate.

DR. MERRITT asked whether he understood that these deposits were thoroughly desiccated before weighing?

DR. KRETZSCHMAR said the deposits were thoroughly dried when weighed.

DR. CHAPMAN would like to ask Dr. Hawley whether he made his experiments with albumen in the test-tube, or in the human stomach?

DR. HAWLEY said that he had made his observations on eight or ten babes.

DR. CHAPMAN did not know whether a discussion as to the efficacy of Pepsin was in order or not.

THE CHAIR assured him that he was in order.

DR. CHAPMAN did not now use Pepsin at all in any shape; but for a long time, when first introduced, he tried it faithfully, but he came to the conclusion, from the results obtained, that it was of no use. Invariably after giving Pepsin, the amount of caseine in the passages was increased, and his patients got along better without it. He took the best Pepsin in the market and found it useless, no matter who the manufacturer was; so he stopped using it, and after that he tried some other plan to accomplish his object. Digestion in test-tubes is not like digestion in the stomach. It makes no matter who the manufacturer of Pepsin is, no man can live by Pepsin alone. The process of digestion does not wholly take place in the stomach. If the nitrogenous materials are not wholly digested in the stomach—are only broken up and digestion is completed by the duodenal glands, the pancreas and liver, he did not see how much could be accomplished by giving Pepsin. He never knew a case of dyspepsia cured by Pepsin. A patient may feel better; they will do the same on bread pills—only give them enough. Given a case of dyspepsia, he defied any living man to cure it with Pepsin; *it cannot be done* because the nitrogenous material is not digested in the stomach. Food is digested in the duodenum as much as in the stomach. Still all over the country physicians prescribe Pepsin; manufacturers send samples put up in all sorts of ways and accompany the same with a letter-book of explanations, making extracts from physiological works; but he wanted, when physiological truths are set forth, the whole truth, not a part of it. Digestion is not completed in the stomach, and no amount of Pepsin will cause complete digestion there. The food must go into the duodenum before it will do good. He believed that every man, woman and child has got to manufacture his or her own Pepsin, and when the time comes when this is impossible, they are not long for this world. You cannot depend on Pepsin alone—he did not care whose manufacture it was, whether it was a 90 per cent. or 15 per cent. specimen, or any other. This whole story about Pepsin is a theory; Pepsin does no good. The stomach must be put in order first. The patient must have good food and air and made to get well. Let him be placed under unfavorable circumstances, and woe unto him!

DR. CHAPMAN was asked whether he had examined the fœces of children as to the globuli which he supposed to be caseine and always found them fœces. He replied that he had not paid much attention to the caseine after it had been passed.

DR. WALKER remarked that these supposed deposits of caseine were found to be fat undigested. He was sure that he had tried to be careful in his experiments, and he was sure that Pepsin, given in the proper way and in proper cases, would do good. Dr. Kretschmar's paper had suggested several points, as, for instance, he (the speaker) believed that Pepsin could be used with an alkaline. He had used Pepsin and bicarbonate of soda with good effect. In cases where there was an excess of gastric juice, he had given a small quantity of bicarbonate of soda before meals, and followed the meal with Pepsin; and the effects proved the benefit of that method of administration.

Another question was as to the cases in which it will be of benefit, and where it will not; for there is no doubt at all but that this distinction can be made. Dr. Kretschmar's paper was incomplete, in that it did not give the results of clinical investigations. He concluded by suggesting the appointment of a committee, to consist of Drs. Chapman, Hawley and Squibb, to investigate the whole subject of Pepsin.

DR. CHAPMAN said if there was any gentleman in the room who had ever cured a case of dyspepsia he would like to hear him.

DR. SANFORD had seen two cases of dyspepsia cured by Pepsin. One case was that of his own wife, who had suffered from most intense dyspepsia. She had fallen off in weight from 135 lbs. to less than 100 lbs., and she had all the other distressing symptoms of this painful disease. She most certainly was benefited by Pepsin, and she took nothing but pure Pepsin. It has been, perhaps, over three years since she was cured; and now, whenever dyspeptic symptoms return she can always immediately be relieved by Pepsin—in fifteen minutes, by full doses, she is cured. And she never took any other remedy that gave relief until she took Pepsin. For six months she took no other medicine, and immediately she began the Pepsin she began to improve. The other case was a friend of his wife who had heard of her case, and who was cured within three months after beginning with Pepsin. A letter recently received states that the cure has been complete. This case also presented most distressing symptoms.

He had no doubt but that three-fourths of the gentlemen present could relate similar cases and results.

For twelve years he had repeatedly prescribed nothing but Pepsin for cases of functional disorders of the stomach with most satisfactory results. Pepsin should be given in doses sufficiently large to be effective. Five or ten grains may have no effect whatever; the dose should be fifteen grains or twenty grains.

DR. CHAPMAN asked whether a person who is accustomed to resort to the Pepsin bottle for attacks of dyspepsia, can be said to be cured. He called that simply relieved.

DR. SANFORD replied that if the doctor referred to the two cases mentioned above, he could say in regard to his wife that she had not taken ten grains of Pepsin in two years. She is not now troubled with indigestion, but it relieves indiscretions of diet. Having been so much benefited by Pepsin, she would naturally fly to the Pepsin at once.

DR. RAYMOND would be sorry to see Dr. Chapman's views go forth unchallenged, inasmuch as he had had some experience with this remedy, especially in the treatment of diarrhoeas of children, and in that class of cases where there is functional disturbance; and he was frank to say that he had almost invariably had good results; and also in a large number of cases of tubercular trouble and of atrophy and malnutrition he had repeatedly found it an excellent remedy. It had also proved to be a good tonic in these cases.

As regards the theory of Dr. Chapman, he did not think that help should be refused the stomach because the digestive process is not completed there. We must not refuse help because we do not help all organs of digestion. Dr. Chapman says he had never seen benefit arise from the use of Pepsin. He (the speaker) had seen most decided benefit. He had seen most decided cures both of dyspeptic troubles and in diarrhoeas of children. He should like to see Dr. Walker's suggestions carried out.

DR. GRAY asked whose preparation of Pepsin Dr. Sanford had used?

DR. SANFORD said he had used Hawley's preparation in both cases. He had used the powder and the powder alone with the addition of a little acid in water.

DR. WALKER said after this discussion, it did seem to him that Hawley's preparation came up to the mark. Foreign preparations do not always do so, and it seemed to him also that it might be well to take into consideration, as a Society, the advisability, if necessary, of recommending Hawley's preparation, after proper tests have been made, or after a series of tests which may be made by the committee proposed, and which he hoped to see appointed this evening.

DR. GRAY asked Dr. Chapman if he used Hawley's Pepsin.

DR. CHAPMAN replied that if he was going to use Pepsin he would use Hawley's.

Pepsin, because he is a friend and neighbor. As for being a member of the proposed committee, it would be out of place because he did not use any of the preparations of Pepsin.

DR. JEWETT had found Pepsin of some service in digesting food outside the stomach in several instances, as of milk for feeding young children.

THE MEDICAL SOCIETY OF THE COUNTY OF KINGS.

SPECIAL MEETING.

A Special Meeting of the Medical Society of the County of Kings was held at 398 Fulton St., on the evening of Thursday, April 22d, 1880, at 8:10 o'clock; the President, Dr. CHARLES JEWETT, in the Chair; DR. WYCKOFF, Secretary, acting in that capacity.

The meeting was called, on the written request of members of the Society, by the President.

THE CHAIR announced the object of the meeting to be to consider the propriety of publishing Dr. Squibb's paper as read at the last April meeting of the Society. He then asked the pleasure of the Society.

DR. SANFORD said that it seemed to him, that, according to the provisions of the By-Laws, the first thing in order was the reading of the Minutes of the last meeting, in so far as they relate to the subject matter of the meeting. He would therefore call for the reading of the minutes of the last meeting relating to the matter in hand.

THE CHAIR stated that they were not yet prepared.

The reading of the stenographer's notes was then called for.

The notes were not present, and the Chair so stated.

DR. SANFORD thought that in order, therefore, to get at the business for which the meeting was convened, a reconsideration of the question voted upon at the last meeting, as to whether Dr. Squibb's paper should be read, should be voted upon.

THE CHAIR asked Dr. Sanford to put his suggestion in the form of a motion or resolution.

DR. SANFORD then moved that the motion passed at the last meeting of the Society, that Dr. Squibb's paper be published, be reconsidered.

THE CHAIR thought that that was not the resolution. To the best of his recollection it was to this effect:

"*Resolved*, That that portion of Dr. Squibb's paper relating to the history of the discussion be not published." That motion was lost. That was the state of affairs if he remembered it.

DR. SANFORD then moved a reconsideration of the motion as stated.

THE CHAIR asked if it was in order to reconsider a resolution at this meeting which had been acted upon at the last meeting?

DR. SANFORD said it seemed to him that there could be no question upon that point, as the By-Laws appeared to sanction such a course. To make it clear he would read that portion of the By-Laws to which he referred, namely:

ARTICLE II, SECTION 2. "After any question has been decided, except one of indefinite postponement, any two members who voted in the majority may, at the same or next stated meeting, move for a reconsideration thereof; without which, no discussion shall be allowed."

THE CHAIR ruled that this was not a stated meeting, and therefore it would perhaps be simpler to bring this question up anew.

DR. GRAY said, if it was in order, and as there were many gentlemen present at this meeting who were not present at the former meeting, he would move that Dr. Squibb's paper be re-read, so that those gentlemen referred to might have a clear understanding of whatever discussion may follow upon the reading of that paper.

The resolution was seconded.

THE CHAIR asked whether that action would be in order, under the call for the meeting.

DR. HUTCHISON asked whether the stenographer had his notes of the last meeting with him?

THE CHAIR stated he had not.

DR. SANFORD inquired whether he understood that the meeting was unable to receive such a motion as that?

THE CHAIR replied that he had not decided; he would leave that to the Society to determine.

DR. KRETZSCHMAR moved an amendment that the objectionable part of that paper be re-read, and that the scientific part be left as it stands for publication.

DR. GRAY accepted the amendment.

THE CHAIR then put the motion, as amended, as follows: That that portion of Dr. Squibb's paper pertaining to the scientific criticism of Dr. Merritt's paper be re-read.

DR. KRETZSCHMAR remarked that the paper of Dr. Squibb, as far as it relates to scientific matter in question, is certainly and undoubtedly a very laborious and excellent paper. He had certainly spent a great deal of time and an immense amount of care in finding out the truth, and he deserved credit from all parties concerned in the matter. The speaker was especially interested, because both he and Dr. Squibb agreed that the tests are not true tests as to the quality of pepsin. So far, very good. *But* Dr. Squibb has no right to read a paper under the title of *The Relations between the Medical Profession and the Business Interests of the Materia Medica*, and then turn it upon personalities. He does not in this paper confine himself to the subject of investigating the qualities of pepsin, but he indulges in personalities and insults. By that course he diverts the scientific aspect of the discussion into unscientific vituperation—by insulting members of the Society. He has insulted not only the members of the Society, but the entire Society; and it is about time to protest against such a thing.

There is another point: Dr. Squibb announced at the last meeting that he should prefer to read his paper by title, and let it be published in *THE PROCEEDINGS*. The Publication Committee three months ago undertook to suppress Dr. Merritt's paper, because it contained personalities. Very well; who is more personal, Dr. Squibb or Dr. Merritt? Dr. Merritt confined himself to what he called a ready test. The speaker proved that he was mistaken, but he never, on that account, indulged in a personal attack in response to the criticism. Dr. Squibb's paper is full of insults to different members of the Society, and it was both loose and unscientific. He had directed his remarks also to the speaker, in his allusion to the tests of pepsin. What he did say was that the tests were found in *Stille and Maisch*.

A MEMBER here undertook to correct the speaker, but

DR. KRETZSCHMAR contended that the allusions to him were so pointed, and he con-

sidered them to be so clearly aimed at himself, that he felt insulted personally. The paper was also an insult against many other members of the Society and against the Society as a body; and he hoped that the Society would be kind enough to strike out that portion of Dr. Squibb's paper which did not relate to scientific matters.

DR. SQUIBB arose for the purpose of making a correction. He did not propose that the paper should be read by title and go to the Committee on Publication. That is not true. He said the hour was late, and, if the Society would permit, he would forego the reading of the paper and publish it himself. He never proposed that the paper should go to the Committee on Publication.

THE CHAIR then announced the motion that the paper, or that portion of it which pertains to the matter of the discussion, should be reconsidered.

On being put by a *viva voce* vote, it was declared by the Chair to be carried; and it was so ordered.

DR. WALKER said he thought the whole paper should be read at this meeting; the first part should also be read, and he so moved.

DR. SANFORD considered that the motion should be changed. The business of the meeting must be confined to that which is stated on the card; none other can be brought forward at a special meeting; otherwise he would be glad to hear the paper re-read—together with that part about which there was no discussion. The other part would hardly come under discussion on account of the By-Laws.

DR. BURGE said the meeting was called in reference to the publication of Dr. Squibb's paper; if that did not bring the whole paper before the Society, he did not know what would. He would like to hear it, *especially* the latter part of it.

THE CHAIR remarked that if there was no objection, it would be considered that the whole paper should be read.

No objections were raised.

THE CHAIR asked by whom it should be read?

SEVERAL VOICES in unison: Dr. Squibb.

DR. SQUIBB said he would be willing to read it. Then rising and walking in front of the President's desk, he asked: Who has got it? Have you got it, Mr. Secretary?

THE SECRETARY replied that he had not.

The Doctor looked around for a moment, at a loss how to read his paper when he had it not, when

DR. HUTCHINS arose, and, taking it from his pocket, handed it to Dr. Squibb, and informed him that the paper was in the hands of the Publication Committee.

Dr. Squibb then ascended the rostrum and proceeded to read *ab initio*.

Upon certain portions of his paper he made running comments. This he had done on several occasions, and so freely on one particular portion that—

DR. GRAY arose to a point of order that the motion called for the reading of the paper, and that it should not be interlarded with explanations.

While these exceptions were being taken Dr. Squibb retired a few steps, and at the conclusion of Dr. Gray's remarks acknowledged that the gentleman was technically correct—he had no right to say one word other than what was in the paper. Then again stepping forward, he proceeded without interruption to the end, and resumed his seat in the audience.

THE CHAIR: The Society having heard the paper read, what is your pleasure in the matter of publication?

DR. SQUIBB then arose and said: And now, Mr. President, if it will at all tend to avoid an acrimonious discussion, which I, as much as any member of the Society, deplore, I would ask somebody to make a motion to allow me to withdraw that paper.

It is not my paper now; it belongs to the Society. I would withdraw it if I could; I cannot do it, because it belongs to the Society; but if any member sees that this is the proper way to get out of the difficulty, and will be kind enough to make the motion, and then have it put to vote, I would like to withdraw that paper from the Society. I am perfectly willing, now, if the Society please, to see Dr. Merritt's paper and Dr. Kretzschmar's paper published, and my own withdrawn; I am perfectly willing to do that. But in all frankness I shall say that if I withdraw it I shall publish it in pamphlet form, and distribute it as I usually do pamphlet papers that I publish.

DR. PROUT saw no reason why the request might not be granted, or why any member of the Society might not have the privilege of withdrawing his own paper, if he thought fit. He would therefore move that the request of Dr. Squibb to withdraw his paper be granted—that he have permission to withdraw it.

DR. HARCOURT began to speak to the motion, but was called to order by the Chair who ruled that the motion was not before the House, inasmuch as it had not been seconded.

The motion was then seconded.

DR. HARCOURT then addressed the House. He had understood from what he had heard that the paper of Dr. Merritt had been written with a motive other than of a scrupulous character—that its ultimate object was that it might be used for advertising purposes. He had also heard that a committee had been appointed to wait upon some legal gentleman and secure his written opinion. If there was any danger of a legal process, he thought that was one reason why it ought not to have been published—that would have settled the whole question.

He had heard of Dr. Squibb's insults and personalities. He had listened attentively to the reading of the paper, and he could not point to one word or sentence which he could conceive to be insulting. He could not find one. To characterize a man's words as insulting and personal is a slipshod sort of argument. Dr. Squibb had mentioned one or two facts, and it remained to be determined whether they are objectionable in the sense of an insult. He had said that in order to ascertain the quantity of pepsin a false method had been used, and proceeded to prove his assertion by showing the results of two months' labor, in disproving the test of Dr. Merritt. He also mentioned that in testing two or three samples, after a negative result had been obtained, another and different result had been obtained. He (Dr. Squibb) therefore characterized that test as insufficient. Now, that is not an unscientific method of refutation. The facts speak for themselves. Where, then, is the insult? It is for them to find out the fact; the Society can then dispose of it.

DR. GRAY asked whether the motion was open for debate?

THE CHAIR replied that the motion before the house was that Dr. Squibb be allowed to withdraw the paper. The discussion, although not to the point, is interesting, and it was desirable to hear all that may be said. The motion was open for debate.

DR. HUTCHISON inquired the object of this meeting.

THE CHAIR replied that it was to consider the propriety of publishing Dr. Squibb's paper as read at the last April meeting of the Society; and the motion now before the house was, that Dr. Squibb be allowed to withdraw his paper.

DR. GRAY said that he would willingly have made the motion desired by Dr. Squibb had it not been for his concluding remark. He should have gone further, and instead of making a motion that he be permitted to withdraw the paper, he should have been willing that he be requested to withdraw only a portion of it. When the doctor informed the Society that it was his intention to publish that paper and circulate the

same broadcast, he hesitated to do what he had intended to do because that was putting it before a wider audience than that which will read it from THE PROCEEDINGS.

The gentleman who had just spoken had misunderstood the object of the meeting. It was not for the purpose of discussing Dr. Merritt's paper, but simply to take into consideration certain points in Dr. Squibb's paper.

Several months ago Dr. Merritt's paper was read before this Society, and there has been no positive proof that Dr. Merritt either read or wrote that paper for unscientific purposes. That paper went to the Publication Committee, and after the lapse of two or three months, not having been published, its publication was requested, which request was not complied with. It was then debated as to whether it was not best to bring the matter up in the Society. He, the speaker, objected, because it seemed to him a personal matter, but on further consideration he became convinced that it was not a personal matter; it was a matter of principle purely, and something that could not be made entirely personal.

Two months ago, therefore, this matter was brought up with great care, with fastidious care, with almost unnecessary care. It was endeavored to impress upon the minds of the audience here, that there was nothing personal in it, and the member who should bring it before the Society was charged to bring it forward in such a manner that the feelings of no man should be wounded. It was endeavored that the language used should be such as to make it publicly plain that there was nothing personal in the matter. It was then brought forward, as is well known to the members of the Society.

A committee was then appointed to inquire into the legal bearings of the Society in this matter, and as to whether the publication of this paper rendered this Society liable for damages. That question was to be submitted to a lawyer, and the report of the lawyer was to be considered final. At the next meeting of the Society no report was brought forward, simply because he was Chairman, and a domestic affliction prevented him from being present.

At the last meeting, the regular April meeting, a paper was read by Dr. Kretzschmar, going over the same ground, using the same quantities of pepsin, and testing by means of weights. It was very evidently brought here for the purpose of disproving Dr. Merritt's assertions, and yet when Dr. Kretzschmar had concluded the reading, he was reluctantly forced to admit that in the main Dr. Merritt's assertions were proved—except in one or two particulars. The paper next following was Dr. Squibb's, in which Dr. Squibb gave his general opinion of the discussion which took place at the meeting, two months previous—characterizing that discussion as utterly worthless and unscientific as it could possibly be—saying that a certain other gentleman of the Society had made use of Merritt's tests in a way that it would have been made use of if it had been published.

That assertion was unwarranted—there was no proof in regard to that other gentleman's motives in reading that paper and what he might have done with it had that paper been published. That paper, in the ordinary course of events, would have appeared in THE PROCEEDINGS of the County Medical Society. It would have gone forth to all the subscribers and to all the readers in the country and in the world—and the discussion which followed would have corrected any bad influence which might have followed. Because the stenographer had given an insufficient report of the debate—

DR. HUTCHINS here interrupted Dr. Gray. The stenographer had made an accurate report of that meeting; and inasmuch as he (the stenographer) had no right to the floor to speak for himself, he (Dr. Hutchins) must speak for him. He was not at fault in this matter.

DR. GRAY continued: At all events they were not published. To resume his remarks, he must say that Dr. Squibb's opinion went forth unchallenged—and if THE PROCEEDINGS are to be made a personal rather than a scientific medium, he would say that that use of them is not germane to their object of discussion. The experiments of Dr. Merritt's were found to agree in the main with the carefully performed experiments of Dr. Kretzschmar by weights. Therefore this quarrel is a triangular quarrel, and Dr. Squibb's paper is entitled to just as much discredit as the other two.

Now the idea in the mind of these gentlemen who called the meeting was that this method of proceeding was unjust; but they also thought that it was an inadvertence. He hoped so. It was an injustice, an injustice which he with others, out of self-respect, should see rectified. There are two or three ways of rectifying it; either that the offensive matter be expunged from Dr. Squibb's paper, or the three gentlemen reading papers withdraw theirs: or, in the third place, that a general account of the proceedings be prepared and published in THE PROCEEDINGS. He was willing to be criticised if he had misstated anything in reference to Dr. Squibb or any other gentleman.

DR. SANFORD said that there was no objection raised, as far as he could recollect, to that part of Dr. Squibb's paper relating to the scientific criticism of Dr. Merritt's test for pepsin. Objections could not be raised on the ground of principle to as full a criticism as he might make. That was a matter of opinion. He had a perfect right to present the scientific part of the subject to the Society. Therefore he considered that part of his paper as perfectly unobjectionable—legitimate and excellent, as well as scientific.

But when he came to the second part of the paper the question seemed to be simply this:

What purports to be a report of what occurred at two meetings of this Society, now officially made, and as he states, hurriedly, and with a great many disadvantages, entirely from his memory, stands in lieu of the printed transactions. The report, as printed, is as follows:

"DR. W. WALLACE, referring to a paper read before this Society some months ago, by Dr. Merritt, upon 'Ready Method of Testing Pepsin,' inquired whether it had been given up or merely postponed.

"DR. HUTCHINS replied that, by direction of the Council, it had been indefinitely postponed.

"The matter was discussed by Drs. Gray, Hutchins, Squibb, Kretzschmar, Wight, Sanford, Westbrook, Walker, Hawley and Merritt; and, on motion of Dr. Gray, it was

"*Resolved*, That a Committee of three be appointed by the President to inquire into the legal aspects of this matter, and report at the next meeting of the Society.

"THE CHAIR appointed Drs. Gray, Sanford and Wyckoff as such Committee, and the Society adjourned."

Now that is the only official report of the meeting of the Society, which occupied about fifteen ordinary pages of Dr. Squibb's paper in its statement; consuming the most important part of his paper, as he says, with a statement of what occurred here in two stated meetings.

Now how shall this Society allow a record of its proceedings to go before the public? There were present there not twenty members, he was sure, in this hall. The presiding officer was called away, and, Dr. Squibb being called to the chair, the paper was read and only twenty members know what occurred at that time.

A comparatively small part of all the members were present. Now we have no official account beyond what has been published and read. How, therefore, shall the

Society allow its official transactions to go before the public? Would it be proper to allow any single member to evolve them out of his own mind? It is that which he objects to, that the Society should be reported from mere memory, and the report to be accompanied by a running commentary. He did not think for a moment but that Dr. Squibb was perfectly honest in his opinions; but what he and his associates did object to was simply the statement of what occurred here from memory. No one can, at this late day—no one could, at the time he wrote his paper, except the stenographer, give an account of what occurred there. The result has been that those who were present could see points in which his account did not tally with their memories, and there is a great discrepancy. There are not only sins of commission but of omission. For instance, it had been stated by at least two members, the why and the wherefore, as to how Dr. Hawley came to read Dr. Merritt's paper. According to Dr. Squibb, it would seem to have been done with a design of afterwards making use of the paper for unworthy purposes. And it would seem to be a strange precedent to permit any one to reproduce from his memory what did occur. The Society must have the stenographer's notes and the Secretary's minutes. But shall we permit a record of the Society's proceedings to go out before the public like that, from memory, as the true report of two important meetings?

If that portion of Dr. Squibb's paper is stricken out, he did not see that there was a person present who would object to the printing of the paper.

THE CHAIR said that this discussion was somewhat irregular; it did not pertain to the question.

DR. SQUIBB remarked that if he had made statements in his paper, he did not see that the Society was responsible for them.

DR. HUTCHINS stated that the reports of the meetings were in the hands of the Publishing Committee, in the hand-writing of the stenographer.

DR. HUTCHISON understood the office of this meeting to be to determine whether Dr. Squibb's paper shall be published or not. The proper way was to vote upon it. He moved, therefore, as a substitute, that Dr. Squibb's paper be published in THE PROCEEDINGS.

THE CHAIR hardly saw how it could be accepted. There was a motion before the house. The motion had been stated by the Chair, and therefore it was before the Society.

DR. DE BOWES suggested that the meeting had been called for a purpose, and that was to determine whether the paper should be published.

DR. PROUT asked whether Dr. Squibb desired to have the motion made by him (Dr. Prout) withdrawn; and after consultation with Dr. Squibb, Dr. Prout adopted the amendment, moving that the paper read by Dr. Squibb this evening be published in the transactions of the Society.

DR. HUTCHISON knew nothing about this meeting before this (Thursday) afternoon, except from rumor. He had heard it said that Dr. Squibb had read a paper which was insulting to the members. That astonished him very much, because it is not Dr. Squibb's habit to insult anybody; he therefore listened with great interest and care to the paper and discussion following it, and he was unable himself to see that the paper contained anything insulting to any member of the Society. It seemed to him to be a dispassionate scientific discussion of the whole subject.

Dr. Squibb had a perfect right to say that the discussion was loose and unscientific—the right to discuss the methods, to discuss the tests in the preceding paper. If a member of this Society is not permitted to criticise others, the Society is worth little in his judgment. He supposed there was no Society which stood higher than the Patho-

logical Society of New York; and the very fact of its high standing is due to this right and practice of criticism. If this Society is going to lay down the principle that papers read here cannot be criticised, one may, with equal propriety, say that the journals have no right to criticise books. If he presented a case from practice, or a new instrument, he did not expect everybody to agree with him in what he says about them. He had no right to assume, if there were contrary views, that they were personalities. For the life of him he had been unable to detect anything that was personal, or that was insulting.

In conclusion he thought this Society would do itself great discredit in refusing to publish every word written in that paper. There has been no paper published in the transactions, in his recollection, that will do the Society greater credit than this one.

DR. HUTCHINS desired to speak in the negative to Dr. Hutchison's proposition. He was a little befogged in reference to the status of this matter, and he was inclined to think the amendment befogs him still further, because he thought Dr. Gray correct, and also Dr. Sanford, in the earlier part of his remarks, where he states that, at the last monthly meeting, the matter was decided that Dr. Squibb's paper was to be published with the personalities stricken out.

THE CHAIR said that the motion was that certain parts be stricken out. Subsequently the motion to strike out was lost.

DR. HAWLEY said he considered himself personally alluded to in several parts of the paper, and placed in a very questionable position. He had asked this Society to protect him, and to order everything relating to the reader of that paper—he was unfortunately the reader—and to the manufacturers of the lauded pepsin, be stricken out, and said that every person here would see that he was the person alluded to; and he therefore asked to be protected by striking that portion of the paper out. It was simply the personalities directed to himself that he asked to be stricken out.

DR. HUTCHINS said what led him to make a suggestion to the effect that while the proposition of Dr. Prout would be in order, the amendment of Dr. Hutchison would seem not the proper course in order to get the paper before the Society again. He asked, therefore, for a reconsideration, and he asked it for this purpose: that, while watching the reading of the paper, and while following the sympathies of those who so earnestly oppose the gentlemen, he failed to find in it the elements of personality which they do. If the resolution to allow Dr. Squibb to withdraw his paper prevailed, the gentlemen would see at a glance that the injustice now surrounding others would be placed on Dr. Squibb; and while these gentlemen are anxious to save Dr. Hawley from any imputation of improper motives, they must see that if this motion of Dr. Prout's prevail, it will be an actual vote of censure.

DR. GRAY desired to say a word in regard to Dr. Hutchison's argument. That gentleman seems to think that if we question the right here of any member to question the opinions of any other member, we do wrong. Dr. Squibb, in other words, has a right to express any opinion, to discuss any paper, in his own way. Equally well every other member of this Society has a right also to put upon record his opinion, and to see that his opinion is not misrepresented. He did not for a moment question Dr. Squibb's right to pass an unfavorable opinion upon Dr. Merritt's paper. We might consider it a question of taste about that. We simply say that, as a matter of justice, if he takes the right upon himself to do that, we have an equal right to see that we are not misrepresented, and that Dr. Hawley is not accused of things of which there is no proof brought before this Society. The proper way is to reconsider the motion to expunge. We must reconsider the question.

The SECRETARY said the President has already decided that it is impossible to do that.

DR. PROUT said he must take exception to that ruling. He did not think that those who draughted the By-Laws ever had any intention of blocking the working of the Society in that way.

THE CHAIR: Dr. Prout appeals from the decision of the Chair.

DR. PROUT said he did not make an appeal, although the Chair might put it in that way. He thought that whatever is necessary to be done could be done under the By-Laws. It was not the purpose of the makers of the By-Laws to block the action of the Society.

THE CHAIR replied that, on the wording of the By-Laws, he had decided the motion to be out of order. He understood Dr. Prout to appeal from that decision. He therefore would put this question: Shall the decision of the Chair be sustained as the judgment of the Society? Members who vote in the affirmative would say "Aye."

None voted.

THE CHAIR: Those of contrary opinion would say "No."

Many voted.

THE CHAIR: The Chair was not sustained, and the motion to reconsider was in order. The motion to reconsider was perfectly in order, provided that motion was made, seconded, and placed on the minutes.

DR. SANFORD moved a reconsideration.

The motion was seconded.

THE CHAIR said it was moved to reconsider the vote of the last meeting, which had the effect to strike out certain portions of Dr. Squibb's paper. The motion was therefore open for discussion.

DR. SQUIBB rose to speak to that resolution, and what he might say might influence the Society in regard to that motion, if he tried to explain a little more fully what does not, perhaps, appear in the paper.

He really in that paper meant no accusation against any one; as far as the points of accusation go, he merely states facts.

Now, the member considers himself accused, because of the statement that the author of the paper was not present to read it; that he (Dr. Hawley) read it; that he was a prominent maker of pepsin, and that his pepsin was lauded as 300% or 400% greater than any other tested. Now, are these facts? Was Dr. Merritt present at that meeting? He was not. Did Dr. Hawley read that paper? He did. Is Dr. Hawley a prominent manufacturer of pepsin? He is. Was his pepsin lauded in that paper? It was. Now, if these are not *facts*, then he had made an accusation. If these are a mere statement of facts, then he could not see the accusation.

THE CHAIR asked pardon of the Society, for, while this motion for reconsideration was in order, the discussion was not in order until the other motions were before the House.

DR. PROUT withdrew his motion, and with it the amendment.

THE CHAIR then ruled that the question of reconsideration was before the House.

Cries of "Question, Question."

THE CHAIR: It is moved and seconded that the motion of Dr. Hawley, made and adopted at the last meeting, be reconsidered. All in favor say "Aye."

Many voted.

THE CHAIR: All of contrary opinion "No."

A few voices heard.

THE CHAIR then ruled that the motion of Dr. Hawley was before the Society for discussion; that is that certain portions of this paper which were considered of a personal character be stricken out and be not published.

DR. SANFORD said that the remarks of Dr. Squibb seemed to him to give the Society just the reason why the point suggested by him a few moments ago apply to this case. Dr. Squibb has stated what occurred. He has reported a stated meeting. He stated certain facts, but omitted to state certain other facts, and this gives a false color to what did occur. The explanation given at that meeting as to how Dr. Hawley read that paper was fully explained. As the matter stands and as presented by Dr. Squibb, it certainly misrepresented, unintentionally, undoubtedly, the facts in the case. Now, if Dr. Squibb's report of what occurred in that part of the paper is to be published, then he insisted that the next time he reads a paper, the Society allow him (Dr. Sanford) the right to give his opinion as to what occurred in the three different meetings. No one has a right to take the place of the Secretary or stenographer and state from memory what was said. He insisted that that part of the paper which relates what occurred at those two meetings had no business there. The Secretary and the stenographer are the medium through which the proceedings of a Society like this should go on the minutes, and not the memory of any individual. If that is not so, then he did not know what the minutes of a Society are good for or what the Secretary's minutes, received and voted upon, are good for at any time.

DR. CORBALLEY understood that the stenographer's notes of these meetings were in the hands of the Publication Committee. If they are here he would like to hear them read, and the Society could judge for itself whether the statements of Dr. Squibb correspond.

THE CHAIR said it seemed rather difficult for the Society to confine itself to this motion.

The Secretary's minutes of the last meeting, taken by the Assistant-Secretary, Dr. Hunt, were here introduced, and a portion of them read by the President as follows:

"*Resolved*, That the personalities referring to Dr. Hawley be stricken out from Dr. Squibb's paper."

It was the intention of the Society, he continued, to reconsider that resolution.

DR. CHAPMAN said: Since he had been sitting here, he had been reflecting in his own mind some way out of this difficulty. It seems, on the one hand, that injustice might be done to one man, and, on the other side, injustice might be done to another. He presumed that both these papers were read in good faith, and he did not think Dr. Hawley had any bad motive; yet the inference may be drawn that he did from the fact that he read the paper, and the laudation is given to a particular pepsin, of which he is the manufacturer. In *that* comes the criticism, and in that regard Dr. Squibb is perfectly just in all that he says in the first part of his paper. But it did seem to him that while he infers indirectly Dr. Hawley's motives, the Society is doing injustice to one member of this Society by its action; and the Society had no right to do it. When he begins to speak about one party reading a paper written by another, the inference is that it would be used for advertising purposes, so that the Society stands in a position of a liability to do injustice to entice one side or the other, and the paper of Dr. Squibb stands mutilated if that portion is to be left out.

Now had the Society, as a society, a right to cast imputation on any member and say that he wishes to use the Society for advertising purposes? It had no legal right to do it.

He saw no way out of the difficulty, except, perhaps, the Society had better not accept either paper; that would save the whole trouble.

In regard to advertising: If these proceedings were to be printed in medical journals, the Society is not responsible, neither are the editors. If certain parties use them for advertising purposes the Society cannot help it; but, unfortunately, in this case,

Dr. Hawley is one of the members, that makes it different ; and the imputation is cast on him that he knew the test a long while ago. The question with the speaker was whether the Society had any right to cast imputation on any member. It had no right to cast imputations, but any man had a right to criticise the paper. All he (Dr. Chapman) could say against pepsin is, *it is not worth anything!* He had a right to say *that*, but, in saying so, he did not want, or intend, to say anything against Dr. Hawley ; he can manufacture all the pepsin he wants to or is able to. Therefore, if that little portion of the paper which seems to imply that Dr. Hawley has done this underhandedly—got Dr. Merritt to write the paper for him to read—he thought the matter might be simplified. He did not think that part should be published.

DR. CORBALLEY insisted that he had called for the reading of the minutes, so far as Dr. Hawley's paper is involved.

DR. GRAY did not think that the question was as to the agreement of the minutes and the memory of Dr. Squibb. The only question was as to the right of Dr. Squibb to take upon himself the functions of the Secretary or the stenographer, and incorporate the result in a paper on pepsin !

DR. SANFORD did not think it was a question of veracity.

DR. SQUIBB said the question was, if the Society order that this paper be changed, or, if any part of it be omitted, will it be any longer his paper? Has he a right to withdraw his name from it in case the Society chose to alter it. He would maintain that if the Society took the paper and altered it, *one word*, that it was no longer his paper, and he should then have a right to withdraw his name from the paper, *and protest against its publication at all*, if he chose.

DR. RAYMOND proposed that no portion of it be stricken out, but that the notes of the Secretary and stenographer be published.

Resolved, That Dr. Squibb's paper be referred to the author, with the request that he erase therefrom all reference to Dr. Merritt's paper, entitled "A Ready Method of Testing Pepsin," and also all reference to the discussion of that paper before this Society ; and,

Resolved, That the paper so amended be referred to the Publication Committee and appear in the next number of THE PROCEEDINGS.

DR. SQUIBB said he would obstruct that amendment by declining to do anything of the kind ; the Society has no right to order him to do anything ; it may request or suggest, but not order.

DR. BURGE said he used the word "request."

The amendment of Dr. Burge was not accepted.

THE CHAIR requested Dr. Raymond to reduce his amendment to writing.

DR. HUTCHINS said if that amendment prevailed, the Society might congratulate itself that the annual fees were laid at \$5.00 for the present year. There was no money in the treasury of THE PROCEEDINGS to cover this extraordinary cost ; and there was no physical ability or moral power sufficient to secure that amount of money. It should be remembered, also, that by a vote of the Society ordering this proposed action, the cost of carrying it out will come out of the funds of the Society.

A MEMBER said the Society would pay for it.

DR. HUTCHINS replied that it was all right ; only understand it beforehand.

DR. GRAY said he did not think Dr. Raymond's motion a good suggestion. If it were carried out the Society would have the stenographer's notes and the Secretary's minutes *plus* Dr. Squibb's interpretation.

DR. RAYMOND here read his resolution as reduced to writing:

Resolved, That the portion of Dr. Squibb's paper referring to Dr. Hawley be not

stricken out; but that the full notes of the stenographer and Secretary of all the meetings in which the subject of Pepsin has been discussed be published in THE PROCEEDINGS.

DR. SEGUR, in seconding Dr. Raymond's resolution, said, as he understood Dr. Sanford, he considered it an injustice to publish Dr. Squibb's paper, without publishing with it the notes of the stenographer and of the Secretary; and that Dr. Sanford further objected, that Dr. Squibb's characterization of a certain discussion as being loose and unscientific, ought not to go forth to the members of the Society forming a part of its Journal, without the discussion which took place going with it. He also objected to Dr. Squibb's statement of fact in regard to Dr. Hawley's reading the paper of Dr. Merritt's; he thought it should not go forth without having the minutes published, showing how he came to read it, thus laying the whole matter before the readers of THE PROCEEDINGS and the reading public in such a way that they may form their own opinion of the whole matter.

It seemed to him that these objections were just, and he, therefore, seconded the motion of Dr. Raymond, as it would tend to heal any division which might otherwise exist in the Society. If by that publication at the expense of the Society that division can be healed, he thought the Society could well afford to do it.

DR. HUTCHINS said Dr. Sanford had alluded to matters occurring at the meeting in December. He had in his pocket the discussion on Dr. Merritt's paper, and no mention is made in that discussion of the explanation as to how Dr. Hawley came to read that paper. It would be necessary to submit whether some statement ought not to be made of this necessary fact.

DR. GRAY said if it was in order he would like to ask Dr. Raymond to accept an amendment. It was that the minutes be not published, but that the Chair appoint a committee of two, one to represent Dr. Squibb, and one to represent the other side; that these two members select a third party, who shall be added to the Committee; and that this Committee take the minutes of the two meetings referred to, and condense them, so as to give the gist of the debate in such a form that it will be practicable for the Society to publish with the money in the treasury.

DR. HUTCHINS said that would usurp the duties of the Council.

DR. GRAY would, of course, ask the courtesy of the Council in this case.

DR. RAYMOND would object, for the reason that, even if that were done, it might not be satisfactory to every one. He, for one, would be willing to help pay the expense. He objected to the amendment.

DR. HAWLEY disliked to plead for himself, and he would not, only that no one else would, and an assertion had been made that he intended to use the proceedings of this Society as an advertisement.

I want to state one fact, he continued, in further exculpation of myself.

It will be remembered by those now present who were here at the last meeting, that Dr. Merritt stated that, on the refusal of the Publication Committee to publish his paper with the names, he took advice as to what course to pursue, and that he had asked five members of the Society, who advised him not to withdraw the names.

He asked one other man's advice; he asked me. I advised him to withdraw the names.

Now does that look as if I want to use that as an advertisement?

When he left my office I believed that he was inclined to do it; but he met others who advised him to the contrary.

In view of all these facts, Dr. Squibb is unkind enough to intimate that I intended to use that paper; that I put it forward; that it was a put-up job! It is all false! I had nothing to do with it, or with the writing of the paper either.

DR. SQUIBB: One word of explanation, in justice to Dr. Hawley.

The inference was not drawn that he intended it for publication until after he actually published it. His advertisement containing the wood-cut slightly modified, which was presented in this paper, is to be found on the cover of the *Annals of the Anatomical and Surgical Society*, and that was published subsequently to the first writing of my paper; and therefore the inference was drawn that it was an advertisement because it was so used as an advertisement.

THE CHAIR then proceeded to re-read Dr. Raymond's amendment:

Resolved, That the portion of Dr. Squibb's paper referring to Dr. Hawley be not stricken out, but that the full notes of the stenographer and Secretary of all the meetings in which the subject of pepsin has been discussed be published in THE PROCEEDINGS.

A vote was taken, and the Chair announced that it appeared to be carried.

A division was called for, and the sense of the meeting was taken by a rising vote.

The SECRETARY counted the votes, and the Chair announced that there were 24 votes in favor of adoption, and 12 votes against.

THE CHAIR was asked what the force of the resolution would be. He replied that its force would be the publication of all papers read and notes of all meetings taken by the stenographer and Secretary, including those of the present meeting.

He then formally declared the motion adopted, and it was so ordered.

THE CHAIR said: As this amendment negatived the original motion, it was hardly necessary to put it.

A motion to adjourn was then made, seconded and carried; and the Chair, at a quarter to eleven o'clock, declared the special meeting adjourned.

—COMMON MIND-TROUBLES, AND THE SECRET OF A CLEAR HEAD.—
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Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρώποισι, κακῶν θελητῆρ' ὀδυναῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—LIBRARY FUND.—Subscriptions have been received from Drs. N. W. Leighton, L. N. Palmer and W. F. Sanford.

—SUBSCRIPTIONS TO THE PROCEEDINGS have been received from Drs. E. N. Chapman, P. J. Dwyer and W. H. Harlin.

—WILHELM F. RAPPOLD died March 14th, 1880, of aneurism of aorta, at Humboldt and North 2d Streets, aged 73 years, 6 months and 15 days.

—THE BEST SOLUTION of salicylic acid, according to the *Louisville Medical News*, is made thus:

- R. Salicylic Acid, ℥i—℥viij.
 Citrate of Potash, ℥ij.
 Glycerin, ℥viiij.
 Simple Elixir q.s. to make O j.

The citrate is dissolved in the glycerin, gentle heat, after which the acid is stirred in and heat maintained until solution is complete. When cool, add simple elixir, and strain.

—DR. JOHN W. HAMILTON died April 4, at his late residence, 437 Classon Avenue, Brooklyn, in the 61st year of his age. He was a native of Pennsylvania, educated at Yale College, and a graduate of the Jefferson Medical College in 1855. He was in the medical service of the United States Army during the late war. He took up his residence in this city about 1865, and in 1869 became a member of the Medical Society. He died suddenly from angina pectoris.

—A NEW BATHING ESTABLISHMENT has been inaugurated at 34 Clinton Street, whereat the luxurious side of cleanliness is actualized by means of Turkish, shower, needle, plunge and other forms of baths. The proprietors, Messrs. Wood & Brooks, solicit the examination of their appointments by the medical profession, to whom they offer special commutation rates.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The May meeting will be held on the 18th, at which there will be presented the following papers:

Peritonitis, by Dr. B. F. Westbrook.

Recent Views on Fracture of Inferior Extremity of Radius, by Dr. L. S. Pilcher.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President</i>	C. JEWETT, M.D., 307 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
<i>Secretary</i>	R. M. WYCKOFF, M.D., 532 Clinton Ave.
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B. A. Segur, M.D., 281 Henry St.	F. W. Rockwell, M.D., 6 Lafayette Ave.
E. R. Squibb, M.D., 36 Doughty St.	L. S. Pilcher, M.D., 4 Monroe St.
J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK. (1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI., Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

DELEGATES TO THE AMERICAN MEDICAL ASSOCIATION.

MEETS IN N. Y. CITY JUNE 1ST, 1880.

Dr. Andrews.	Dr. Fowler.	Dr. A. Otterson.	Dr. Skene.
" Armor.	" Griffiths.	" Pilcher.	" Vanderveer.
" Baker.	" Hopkins.	" Reese.	" Wallace.
" Barber.	" Hutchins.	" Rockwell.	" B. F. Westbrook.
" Bartlett.	" Hunt.	" Rushmore.	" Wight.
" Bodkin.	" Mason.	" Sanford.	" Wyckoff.
" Byrne.	" Mattison.	" Shaw.	" Wunderlich.
" Catlin.	" Mitchell.	" Sherwell.	

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Drs. R. M. Buell.
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

JUNE, 1880.

No. 4.

REPORT ON MEASLES.

BY THE COMMITTEE ON HYGIENE OF THE MEDICAL SOCIETY OF THE COUNTY OF
KINGS, BROOKLYN, N. Y.

The history of medicine shows that for several hundred years previous to the time of Sydenham, measles, scarlatina, varioloid and variola were not clearly diagnosed as separate diseases, and it was not till 1793 that the line of distinction was clearly drawn by Dr. Withering between typical scarlatina and typical measles. The line has been drawn since then so closely that scarlatina has been considered the grave disease, and measles a disease of little import—one eminently adapted to domestic treatment, and not worth dwelling upon by medical professors. Comparatively little has been written upon measles.* A search through the various numbers of the Index Medicus discovers but two or three papers bearing on the subject.

The experience of physicians within the last ten years, with the knowledge afforded by improved methods of obtaining and registering statistics adopted by our best health boards, clearly show that measles is sometimes fatal—that the typical rose-colored crescentic eruption is not always present, that the eruption may have a scarlet-like hue, or it may be masked by the concurring eruption of scarlatina, that the complications

* This is especially noticed in the literature of this country.

and sequelæ of measles are more numerous and more injurious than has been pointed out; that it is not easy to distinguish a severe case of rubella from a mild case of scarlatina, any more than a mild case of varioloid from a severe case of varicella.

MORTALITY.

That measles is fatal at times will be seen from the following table,* covering eleven years, and showing the comparative mortality in this city from measles, scarlatina, diphtheria and pertussis, the chief zymotic diseases, especially of infancy, all allied at least as to contagion:

	1869	1870	1871	1872	1873	1874	1875	1876	1877	1878	1879
Measles... ..	262	62	67	59	69	90	8	159	10	67	40
Scarlatina... ..	348	220	522	327	314	479	304	352	730	363	344
Diphtheria... ..	139	110	157	184	252	580	965	812	778	544	689
Pertussis... ..	193	71	110	96	138	130	181	190	118	195	204

During the first quarter of 1880 there has been 51 deaths from measles.

MORTALITY AT DIFFERENT AGES.

Measles is essentially a disease of childhood, though in certain communities and epidemics it attacks adults, and then proves itself more injurious than in children. An examination of 306 death certificates in the Brooklyn Health Office reveals: under 1 month of age, 1 death. Between 1 month and 12 months, 69. Between 1 and 2 years, 121; 2 and 3 years, 47; 3 and 4 years, 22; 4 and 5 years, 20; 5 and 6 years, 6; 6 and 7 years, 8; 7 and 8 years, 6; 8 and 9 years, 4; 9 and 10 years, 1; 10 and 11 years, 1; 31 years, 1.

Schutz† states that children 6 to 8 years old furnish the greatest contingent of fatal cases.

Schiefferdecker's ‡ tables tell us that in London in the ten years—1856 to 1866—there died of measles 19,937 persons; 18,666 in the first five years of life. From 5 to ten years, 1,076; 10 to 15 years, 84; and above 15 years, 111 only.

Passow§ writes that in Berlin in four years the mortality was in 1st year, $\frac{24}{100}$; second year, $\frac{31}{100}$; 20 to 25th, none; 30 to 35th, a very few; above 35 years, 1 only.

* The Committee here acknowledge the kindness extended by the Sanitary Superintendent and Registrar during its investigations after statistics.

† "Würzburger Correspondenz," 1858. p. 37.

‡ "Einfl. d. Acuten Haut-ausschl. auf der Kindersterblichkeit," 1870.

§ "V. H. Jahresbericht," 1869, B. I., p. 285. "Vierteljahreschriften für öffentliche Ges.," B. II., S., 158.

Ranke* gives the mortality at Munich from 1859 to 1868. Mortality for first 5 years, 245; first year and under, 195 ill, 70 died. 1 to 5 years, 119 died. 15 years and upwards, 185 cases, and 11 deaths.

Voit† states, as to the mortality in Wurzburg from 1842-1871, that of children under 1 year, 88 were ill, 21 died, or 2.38. From 1 to 5 years, 367 ill, 15 died, or .04. From 5 to 15 years, 289 ill, 3 died, or .01.

Monti‡ gives the following statistics at the Vienna Children's Hospital for 1864-5-6-7: 372 cases, 98 deaths. Under 1 year, 16 were ill, 6 died. 2 years old, 35 ill, 21 died. 3 years, 52 ill, 26 died. 4 years, 47 ill, 13 died. 5 years, 39 ill, 10 died. 5 to 6 years, 43 ill, 9 died. 6 to 7 years, 38 ill, 6 died. 7 to 8 years, 33 ill, 4 died. 8 to 9 years, 32 ill, and 3 deaths. Above 9 years, no deaths.

Geissler§ gives the history of an 1861 epidemic, viz., 1,754 cases, 63 fatal. Under six months 13 sick, no deaths. 6 months to 1 year, 99 ill, 2 died.

1 to 2 years,	221 cases,	19 deaths.
2 to 3 years,	264 "	26 "
3 to 4 years,	226 "	7 "
4 to 5 years,	204 "	6 "
5 to 6 years,	187 "	1 "
6 to 7 years,	151 "	1 "
7 to 8 years,	144 "	1 "
8 to 14 years,	227 "	0 "

Spiess (op cit) gives the details of the epidemic at Frankfort in 1860-61:

YEAR.		CASES.		DEATHS.		PER CENT.
1	45	8	18
2	156	15	10
3	204	9	4.4
4	186	3	2
5	243	4	2
to 10	954	7	7

Kellner|| reports 43 deaths at the following ages: 1 year, 8; 2 years, 18; 3 years 5; 4 years, 4; 5 years, 4; 6 years, 1; 7 years, 1; 8 years, 2. None above 8 years.

Faber¶ tells us that in the Schorndorf epidemic of 1827-28 the general mortality in 2,100 cases was 1.8 per cent.

* "Jahresbericht für Kinderheilkunde," 1869, II., p. 34.

† "Jahrbücher für Kinderheilkunde," 1872, New Series, S., p. 256.

‡ Idem, 1864, I., p. 52; 1865, VII., p. 52; 1869, N. S., II., p. 77.

§ "Küchenmeister's Zeitschrift," für 1862, I., pp. 353-404 et passim.

|| "Frankfurter Jahresbericht," 1858, II., p. 36.

¶ "Würzburger Correspondenz," 1852, XXII., p. 221.

The mortality of measles varies even in the same localities in different epidemics, without a known cause for the difference, and is not always the greatest in the epidemics in which the majority of cases are severe.

Geissler (op cit) found in two epidemics that of all the children who died from any disease 2.1 per cent. died from measles. "The epidemic of 1861 proved a severe one, but only 3.5 per cent. died. In two Hungarian epidemics at Lippe. In 1856, over 50 per cent. perished, while in 1863 3 per cent. perished (Ziemssen Enc., vol. II., p. 114).

Ranke (op cit) states that in four epidemics in Munich the mortality varied from 0.7 to 2.7 per cent.

Veit (op cit) in the Wurzburg statistics from 1842 to 1871, out of 851 cases of measles there were 39 deaths—to $4\frac{1}{2}$ per cent.

Koestlin * gives the percentage in Stuttgart from 1852 to 1865 as 1.8, and in Frankfort in 1858 as 2.4.

Voit† says the mortality at the Kinderklinik at Wurzburg, in 1863, a very severe epidemic, was 10.5 per cent.

Meier‡ gives the mortality in Baden for 1818 to 1824 as 5.4 per cent.

Carroll§—in the epidemic at Sydney (New South Wales), mortality was 6 per cent.

In a Scotch epidemic || the mortality was as high as 9.7 per cent. In an epidemic at Zolkier¶ (Russia) in 1840, there were 1,519 cases and 196 deaths.

In an epidemic at Nagold** nearly 10 per cent. died. In an epidemic in the village of Oltorf, †† out of 95 ill 10 died, being nearly 10 per cent.

The rate of mortality from measles does not seem to depend entirely upon the density of the population of a district, or its apparent unhealthiness, if we may judge from the following table, showing the mortality in the wards of Brooklyn from measles, in conjunction with that from scarlatina, diphtheria and pertussis, for the years 1874-5-6-7-8-9, and the first quarter of 1880:

* "Archiv. der Ver. für Wissenschaftliche Heilkunde," 1866, II, p. 338 und "Würzburger Correspondenz" für 1856, p. 31; 1862, p. 197.

† "Jahrbücher für Kinderheilkunde," 1872, V., p. 256.

‡ Quoted in "Ziemssen," II., p. 114. Am. Ed.

§ "Oestreich. Jahrb. für Pädiatrik," 1870, I., p. 61.

|| "Schmidt's Jahrb.," No. 88, p. 535.

¶ "Oestreich. Med. Jahrb.," 1843, B. 45, p. 263.

** "Würzburger Correspondenz," 1858, p. 37.

†† "Würzburger Correspondenz," 1838, VIII., p. 13.

DEATHS FROM MEASLES, SCARLET FEVER, DIPHThERIA AND PERTUSSIS,
FROM 1874 TO APRIL 1st, 1880, BY WARDS.

1874.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Measles.....	2	1	3	1	2	9	11	3	3	4	2	4	3	1	4	3	13	5	2	4	3	4	2	..	1
Scarlet Fever.	3	16	13	12	36	22	28	26	24	33	24	20	10	14	13	32	52	8	5	18	36	22	5	1	6
Diphtheria...	28	16	32	12	11	131	21	24	9	37	22	34	19	19	9	27	17	9	24	16	17	16	7	3	20
Pertussis.....	6	3	5	..	3	12	7	4	..	15	8	3	2	4	5	6	17	3	6	2	6	5	3	3	4
1875.																									
Measles.....	1	1	2	1	1	1	1
Scarlet Fever.	10	5	9	9	13	10	23	8	10	10	11	5	10	15	15	22	20	19	6	22	23	11	8	7	3
Diphtheria...	19	27	55	16	19	74	32	29	29	106	31	59	30	37	35	63	65	47	32	33	30	23	24	13	46
Pertussis.....	3	2	4	5	6	14	14	2	6	14	7	5	5	7	9	19	14	7	8	5	9	6	3	2	5
1876.																									
Measles.....	4	4	5	4	9	15	8	3	5	11	5	18	2	3	10	11	9	6	5	5	8	3	2	..	4
Scarlet Fever.	5	5	12	4	11	23	9	9	3	20	15	42	12	12	15	23	20	12	9	16	21	9	5	8	32
Diphtheria...	25	10	27	11	23	55	34	17	52	56	38	49	31	55	36	31	29	31	18	46	45	27	16	21	29
Pertussis.....	4	5	3	8	12	12	7	2	7	16	10	11	6	4	8	19	16	4	8	4	13	3	2	1	5
1877.																									
Measles.....	..	1	2	1	1	3	..	2	1	1	1
Scarlet Fever.	11	18	11	15	38	33	35	29	16	29	29	57	28	45	37	51	45	30	27	9	48	23	16	3	37
Diphtheria...	10	10	22	8	19	35	41	12	36	33	17	36	28	52	41	64	29	36	27	18	59	28	36	9	72
Pertussis.....	1	2	4	..	1	10	5	8	1	4	1	8	3	6	11	17	7	6	6	6	6	3	2
1878.																									
Measles.....	2	2	..	4	1	14	3	5	4	4	1	3	3	7	2	3	4	..	1	4
Scarlet Fever.	12	4	26	2	11	41	15	18	4	18	9	3	16	6	8	24	36	3	21	9	36	27	6	3	5
Diphtheria...	10	2	10	3	9	17	37	8	7	27	19	11	32	60	35	69	34	39	23	9	32	17	16	3	15
Pertussis.....	5	1	11	7	3	13	22	4	16	10	7	8	8	9	8	12	13	5	8	1	6	3	4	6	5
1879.																									
Measles.....	..	1	2	4	5	4	6	2	..	5	3	1	6	1	..	1	..	1	1	2
Scarlet Fever.	9	18	11	10	10	25	14	8	9	15	10	9	11	11	14	25	23	16	6	15	23	14	12	3	23
Diphtheria...	15	18	15	12	36	51	87	12	36	32	49	18	16	25	24	47	17	14	35	22	57	23	8	1	19
Pertussis.....	6	6	10	5	16	13	5	5	1	17	13	21	4	8	7	6	4	8	5	4	11	8	2	2	7
1880.																									
[First Quarter.]																									
Measles.....	2	2	4	2	6	2	2	..	2	1	2	2	1	5	3	2	2	1	3	..	4	2	1
Scarlet Fever.	..	2	2	3	1	1	..	2	3	..	4	2	5	6	5	4	6	5	5	2	1	..	3
Diphtheria...	7	5	11	6	17	11	18	5	4	7	12	6	5	3	3	12	5	6	7	4	15	15	3	1	3
Pertussis....	3	2	4	9	1	2	..	2	..	1	2	2	..	1	2	1

It will be seen by the foregoing table that in no ward has any of the above-named diseases had the largest mortality for any *two* or *more consecutive* years.

If we examine the following summary of deaths for the years 1874-5-6-7-8-9, and the first quarter of 1880, it will be seen that in the 6th Ward, the most densely populated one in the city, there has been the greatest mortality from measles, diphtheria and pertussis, but *not* from scarlet fever. This distinction is reserved for the 21st Ward. Why has not scarlatina asserted itself more strongly in the 6th Ward, if

"The 20th, 7th and 23d Wards have the best health, are on high ground, well drained, have the best class of houses, and are free from tenements."

Why did diphtheria spread as it did in 1875 and '76? "The 24th Ward, a high, healthy ward, had many deaths, while the 16th, low and overcrowded, had many less. If the disease springs from contagion or uncleanness this last was just the ward for its propagation."

"The Board has demonstrated, to its entire satisfaction, that diphtheria is propagated both by contagion and feculent surroundings."

In the 1st, 6th, 8th and 12th Wards, Dr. Colton believed that prominent causes of general mortality were defective drainage and sewerage. He adds: "Out of 580 deaths from diphtheria, in 1874, 217 were in my district, 131 in the 6th Ward. Though giving much thought to the problem, I have been unable to discover any law governing this distribution, or to explain why the 6th Ward has been so great a sufferer. * * *

"The public schools have been suggested as being natural distributing depots of the poison.

"The spread of the epidemic cannot, it seems to me, be charged on the public schools. In No. 27 one death has occurred thus far; no other case has been known among the pupils, of genuine diphtheria."

Dr. Corbally believes that "the nature of the ground, more than the density of the population, increases the mortality."

Dr. Raymond, in charge of the 3d, 9th, 10th, 22d and 24th Wards, states that scarlatina and diphtheria seemed to be aggravated by the sewerage, or innocently called *sewer-sand*, deposited by contractors upon the streets; that Dr. Chas. Jewett had ascertained that each cart-load of such material contained 15 to 30 lbs. of excrementitious matter. He suggests that measles should be controlled by means similar to those employed in scarlatina and diphtheria. This is the only mention of the advisability of quarantine for measles that the Reports furnish.

The Reports referred to show the *general* mortality in the various wards to be as follows: largest in the 16th (47 per cent.), then in 18th, 8th, 6th, 12th, 9th, 14th, 21st, 2d, 15th, 19th, 22d, 11th, 5th, 10th, 17th, 13th, 4th, 1st, 20th, 7th and 3d (19.33 per cent.) Our summary has shown a different arrangement in the case of contagious diseases.

Different cities vary much as to the rate of mortality from measles—and without a known reason—as the following table* shows:

Does the season of the year exert any marked influence on the spread and mortality of measles?

* Schiefferdecker.

City.	Years.	Per cent. of all deaths.
London.....	11.....	2.7
Frankfort-on-Main.....	12.....	1.2
Königsburg.....	12.....	0.92
Geneva (Canton).....	13.....	0.66
Stuttgart.....	15.....	0.63
Manchester.....	7.....	0.58
Berlin.....	18.....	0.38

The returns are not uniform on this point. In Austria a certain town had two epidemics, one in the winter, the other in the following summer. For the cold months the mortality was 2.29 per cent., for the hot months, 6.29 per cent.

Voit, in his records of the Kinderklinik, at Wurzburg, found that in an average of 30 years, the mortality from measles was, in spring, 11.5 per cent., summer, 2.5, autumn, 0.4, winter, 12.7.

Passow, in his records of the mortality at Berlin, during 1863-7, inclusive, finds that in spring it was 11.9 per cent., summer, 13.3, autumn, 33.4, winter, 41.4.

COMPLICATIONS AND SEQUELÆ OF MEASLES.

These are quite numerous ; some are especially associated with the poison of measles, but the number is very small ; others depend upon defective surroundings ; some are alike common to all of the contagious or infectious diseases.

The mortality and prognosis of measles will be found to follow more or less closely these complications and sequelæ.

Kellner* has shown that the complications appear as follows : 1st year .44 of cases, 2d, .52, 3d, .34, 4th and 5th, .21, 6th to 10th year, .21, 10th to 15th, .18, 15th to 20th year, .18, above 20 years .10 ; but Spiess, in the same journal, for 1867, gives a much different result : 1 year, .07, 2 years, .42, 3d, .25, 4th to 5th year, .15, 6th to 15th year, .12, 15th to 20th year, .4 ; all others .26.

COMPLICATIONS.

a—Eye and Ear. Conjunctivitis is usually seen in the beginning of the eruptive stage, and is generally a simple irritation, attended by redness, slight photophobia and lachrymation. It may appear as a more or less severe catarrh with considerable chemosis. In exceptional cases the secretion may be purulent, and is then more or less contagious.

Where there is much eruption on the eyelids, there has been noticed a strong tendency to blepharitis ciliaris. Two such cases have been no-

* "Frankfurter Jahresbericht," 1858, II., p. 36.

ticed in the last epidemic (Sizer). Dr. Edw. Petzold, of New York, has seen a case of blepharitis fungosa. Rare eye lesions are phlyctenular conjunctivitis, abscess of the lids and kerato-iritis, especially in strumous children. The rarity or prevalence of eye complications, as of other complications, seem to depend, according to the testimony of physicians, largely upon the degree of care exerted in the various cases of measles. "No eye complications," writes Dr. Jewett, "when patients are confined to dark rooms till the photophobia has subsided."

Although the eruption of measles generally appears first on the pharynx and is sometimes attended by much inflammation, ear troubles are not common as complications, otitis media being the main one. As sequelæ, ear affections are of themselves sometimes dangerous to life.

b—Lesions of the Skin. Though lesions of the skin have been found, they are rare. Those ascertained to exist were, urticaria,* erythema, scarlatinoides, acute lichen,† herpes facialis,‡ bullæ of pemphigus,§ ecthyma,|| impetigo and urticaria,¶ gangrene** (2 cases), and emphysema facialis after gangrene.†† Dr. C. L. Mitchell records a case of purpura. Prof. Gerhardt,‡‡ of Wurzburg, observed in one case for one day preceding an eruption of measles an exanthem over Scarpa's triangle only.

c—Nasal, Pharyngeal and Pulmonary Tracts.—Naso-pharyngeal catarrh, epistaxis, bronchitis simple and capillary, catarrhal pneumonia, rarely the croupous form, are the commonest forms of complications. Gangrene of the lung has been observed in *two* cases.§§ According to Barthez and Rilliet,||| in an examination of the bodies of persons who had died from measles, one-half had ulcerations and erosions of the larynx, especially on the vocal cords. These erosions have been observed during life by Gerhardt (op. cit.)

Prof. Armor's experience indicates "more complications this year (1880) than ever before—mainly laryngeal and bronchial, lobular pneumonia, diphtheria, acute congestion of the kidneys, otorrhœa, and at times, *typhoid symptoms*." Has not the carelessness of people as to the leaving

* Meyer Hoffmeister, "Schweitz. Zeitschrift, f. med.," 1849, p. 468.

† Hauner "Jahrb. f. Kdrhknde." VI., pp. 119, 150.

‡ Thomas in "Ziemssen Cyclop."

§ Krieg, "Canstatt's Jahrbücher," 1843, p. 219.

|| Thore and others "Schmidt's Jahrbücher," 1861.

¶ Salzmann. "Würz. Corresp," 1860, 1862.

** De La Faye "Jour. f. Kndrkrkht.," pp. 40, 41.

†† Haslop quoted in "Ziemssen."

‡‡ "Jena Zeitschrift," III., p. 118, "Deutsche Archiv., für Klin. Med.," XII.

§§ Steiner und Neureutter "Prager Viertljhrschft.," 84, pp. 92, 106, f. '73.

||| "Journal für Kinderkrankheiten," 1870, X., pp. 359, 432.

off of flannel under-clothing, due to the mild winter, and early but variable spring, had much to do with the frequency of these complications, rather than any inherent severity in the epidemic itself?

d—Complications in the Digestive Tract.—Among these are stomatitis,* aphthæ,† marked salivation,‡ parotiditis§ and angina, sometimes resulting in parenchymatous tonsillitis. Severe gastritis is rare, but intestinal catarrhs are not uncommon. Diarrhœa sometimes lasts for over a week, and proves dangerous in nurslings especially, and in the summer; yet it does occur in the cold weather. Dysentery sometimes occurs even in winter and in severe epidemics, and among adults may be a cause of death.

e—Complications of the Nervous Systems.—The most common have been meningitis, simple and tubercular, encephalitis, apoplexy, hemiplegia, hydrocephalus, acute and chronic, cerebral thrombosis, mania, chorea, tetanus, eclampsia, facial and intercostal neuralgia and of the motor-oculi muscles.

f—Miscellaneous Complications.—Endo and pericarditis, tuberculosis, pleuritis, ascites, albuminuria (frequent), peritonitis, hypertrophic spleen, icterus (several cases), parenchymatous nephritis, œdema of the extremities, lymphangitis, abscesses, sclerema, intermittent fever, acute rheumatism, hæmorrhagic diathesis (observed by Trousseau), morbus maculosus (1 case), rachitis, diphtheria (rare)—usually of pharynx or larynx, but has been seen on eye-lid, conjunctiva, prepuce and vulva. Gangrene of the mouth and noma are very fatal, especially in strumous, debilitated or rachitic children. Gangrene sometimes appears elsewhere, as upon the fore-arm.¶ In an epidemic at Sydney, there were several cases where the whole epidemis of the face and thorax was lost in 24 hours.

SEQUELÆ OF MEASLES.

Sequelæ, other than a temporary loss of muscular vigor and nerve tone, are rare, *i. e.*, in children *healthy* at the time they were attacked by measles. They are oftenest observed in cases of impoverished blood and where home surroundings are bad. Scarlatina does not make such distinctions, therefore is a much graver disease in the community at large. Sequelæ, when observed, have been chronic pustular eruptions of the skin, furuncles, ozœna, chronic conjunctivitis, blepharitis ciliaris, hordeolosis, keratitis, sometimes with loss of the cornea, paralysis of

* Weil, Du Fével, Thore.

† Hartmann, et. al.

‡ Erichson in Willan & Bateman's "Diseases of Skin."

§ Weil & Heyfelder.

¶ Meyer Hoffmeister, op. cit. De La Faye, op. cit.

accommodation with strabismus convergens (rare), capsular cataract (very rare), chronic aural catarrh (common), suppuration after perforation of the membrane, attended by deafness, or danger to life by extension of inflammation to the brain. Chronic hypertrophy of the tonsils (very common), chronic catarrh of the larynx, trachea and bronchi, asthma, chronic hypertrophy of the parotid gland, rarely periostitis and caries. In children cheesy pneumonia may result and be followed by tuberculosis, or if strumous, bronchial glands are apt to become cheesy. Chronic entero-colitis and diarrhoea, intestinal ulcers and resulting stenosis, cardialgia and ascites may result. One case of vesical calculus is reported as a sequel.*

Measles occurring in the course of any disease renders it more dangerous. A simple bronchitis is apt to become capillary and end in pneumonia or tuberculosis. One case, in which there was aortic aneurism, died of suffocation.† Chronic diarrhoeas do badly, and a simple diarrhoea is converted readily into cholera infantum.

For the purposes of comparison, the following table is given, showing the complication or "secondary" causes of death in—deaths from measles, scarlet fever and pertussis, compiled from an examination of individual death reports in the Brooklyn Health Office.‡

It will be seen that the complicating causes of death are proportionately about the same in measles and pertussis, and that there are but few deaths due to the measles poison *per se*.

While it is possible to gather together, as your Committee has done, many causes of mortality from measles, yet the records of well-managed institutions and families show few complications and few deaths. Dr. Leaming writes: "There have been *several* epidemics of measles in the P. E. Orphans' Home and Asylum (with which I have been and am connected). I am not aware that there has been a single death nor a serious complication." This institution is mentioned especially, as it is a model one. False economy in management does not strangle its usefulness.

EXEMPTION FROM MEASLES.

Experience has shown that individuals may have measles twice, thrice, or as Flint says, four times. Tweedie, and after him Sir Thomas Watson, stated that rubeola sine catarrho afforded no protection against a subsequent attack of measles, but there is evidence that second attacks do occur in persons in whom the first attacks were, to all appearances, typical. One of your Committee has notes of a malignant case, beginning with convulsions, attended for some days by delirium, and impair-

* Coulson.

† Walz.

‡ See next page.

Certificates Examined..	366	366	228	282	
Complications Registered.	Measles.	Scarlet Fever.	Diphtheria.	Pertussis.	
Broncho Pneumonia...	1				1. Dentition. Infl. Lungs—then Cerebral Effusion.
Cheesy “ ..	1				1. Remittent Fever—then Meningitis.
Pneumonia.....	82	6		30	1. Bronchitis. Erysipelas of lower extremities.
“ Chronic....	3				Convolutions. April, '74—Dr. Burge, Cath.
Bronchitis.....	20		1	7	Segelke 1st W.
“ Chronic.....	1				2. [Metastasis]?
“ Acute.....	2				2. Pneumonia and Convolutions.
Capillary Bronchitis...	7		1		1. “ “ Diphtheria.
Congestion of Lungs...	25			5	1. Diarrhœa and Bronchitis. Feb., 74, 3 y'rs old.
Edema of Lungs.....		3	1	14	1. Spasm of Glottis and Croup.
Phthisis Pulmonalis....	1	2		2	1. Bronchitis and Convolutions.
Tuberculosis.....	1				1. Convolutions and Asphyxia.
Pleurisy.....	2				1. Asphyxia and memb. Croup.
Laryngitis.....	(Feb. '69.)		2		1. “ “ Edema Cerebri.
Pericarditis.....		2			1. Exhaustion and Intestinal Catarrh.
Cerebral Paralysis....		1			
Meningitis.....	8	1		4	15
“ Tubercular...	4				
Convolutions.....	33	22	7	36	
Congestion of Brain....	15	13		7	
Epilepsy				1	
Nervous Debility				1	
Hydrocephalus, Chronic	6	3	1		
“ Acute...	1				
Paralysis.....		2			
“ of Heart.....			1		
“ Malignant....		9	1		
			(Strangulation.)		
Throat Complications..		5			
Remittent Fever.....				2	
Typhoid “	1		1		
Typhus “				1	
Typhoid Form.....		1			11
Scarlet Fever.....			6	1	
Diphtheria.....	3	11		1	
Pertussis.....	1				
Measles.....			2	1	
Croup.....	1	3	22		
Mumps.....		1			
Marasmus.....	6	1		5	
Exhaustion		5	1		
Asthenia.....					1. Extension to Lungs.
Rheumatism.....		1			1. Remittent Fever and Ulc. Mouth.
Purpura.....		1			2
Dentition.....	2			1	
Cholera Infantum.....	1 yr Jan. 74				
	5			1	
	(July, 69,				
	16 W.)				
Diarrhœa.....	4		1	3	
Serous Diarrhœa		1			
Gangrenous Stomatitis..	1				
Acute Dysentery.....	1				
Dysentery.....	1				
Asthenia or Exhaustion.		4	12	1	
Enteritis.....	2				
Peritonitis.....	(April 74.)			1	
Ulceration of Bowels...	1				
“ “ Throat..		5			
Uræmia.....		6			
Albumenurhea.....		6	1		
Asphyxia.....		1	11	1	
Blood Poisoning	3	1	4		
Acute Nephritis.....		5	1		
Dropsy.....	1	2			
Anasæra.....		12	2		12
Abscess.....	1	1		12	
Adenitis.....		1			
Pyæmia	1				
Malignant Ulceration..	15		2		
Gangrene.....		1	1	1	
Scrofula.....		11			
	263	150	82	139	

MEASLES.

CONSECUTIVE COMPLICATIONS.

1. Dentition. Infl. Lungs—then Cerebral Effusion.
1. Remittent Fever—then Meningitis.
1. Bronchitis. Erysipelas of lower extremities.
- Convolutions. April, '74—Dr. Burge, Cath. Segelke 1st W.
2. [Metastasis]?
2. Pneumonia and Convolutions.
1. “ “ Diphtheria.
1. Diarrhœa and Bronchitis. Feb., 74, 3 y'rs old.
1. Spasm of Glottis and Croup.
1. Bronchitis and Convolutions.
1. Convolutions and Asphyxia.
1. Asphyxia and memb. Croup.
1. “ “ Edema Cerebri.
1. Exhaustion and Intestinal Catarrh.

SCARLET FEVER.

1. Croup and Congestion of the Brain.
1. Nephritis and Hydro-Pericardium.
2. Diphtheria and Nephritis.
1. Diphtheria and Carbuncles on the leg.
1. Exhaustions after Pr.
1. Pneumonia and Infla. of the Bowels.
1. Followed by Measles—then Diphtheria.
1. Pneumonia and Dropsy.
2. Cour. without Exanthem.

DIPHTHERIA.

1. Extension to Lungs.
1. Remittent Fever and Ulc. Mouth.

PERTUSSIS.

3. Dentition and Convolutions.
1. Measles and Debility.
1. Cap. Br. followed by Convolutions.
1. Convolutions and Diarrhœa (April).
1. Pneumonia, then Tuberculosis.
1. Cerebral Edema.
1. Pneumonia and Convolutions.
1. “ “ Dysentery.
1. Bronchitis and Dentition.
1. “ “ Exhaustion.

ment of mental condition for nearly two months, in whom typical measles was diagnosed two years before by a competent physician.

Drs. Frank and Williams, fifty years ago, and Dr. William B. Cheadle,* a year ago, suggested as an explanation of the recurrence of the disease that there might be two distinct diseases (apart from Roetheln), each of which confers an immunity from a second attack of the same variety, but affords no protection against the other; but there are no facts or arguments to sustain this assumption.

The mass of evidence shows that a person who has had one attack of typical measles is not *likely* to have a second attack, and that rubeola sine catarrho is a form, not variety of the disease, which fails to afford any considerable degree of exemption.

There seems to be such a thing as acclimation in measles. It has been noticed that when measles is first introduced into a country it is almost always exceptionally severe. Thus the well-known epidemic among the South American Indians, in 1749-50, destroyed 30,000. Similarly in Madagascar, in 1806, 5,000 natives died in one month. While on the contrary, Col. J. J. Woodward† states that during the late war, in our army, out of 21,676 cases of measles, 2.5 per cent. only died of causes referable to the disease, "pure and simple." * * *

"The fatal *epidemics* were typhoid in character."

In view of the facts that measles is not *always* trivial, that it is *sometimes* fatal, and during the first quarter of 1880 has had 80 deaths, or if not fatal may be followed by unfortunate sequelæ—is it desirable to insist upon a rigid quarantine as was suggested by the present Sanitary Superintendent, Dr. Raymond, in his excellent paper, published in the May number of THE PROCEEDINGS? Your Committee believe that it is not, for the following reasons: 1st. That while measles may prove fatal, yet in well-regulated families it is not probable. 2nd. That the period of incubation is very variable, not always made out. 3d. That the contagiousness of measles is greatest in the earliest stage—sometimes before the disease is suspected. 4th. That a "rigid" quarantine is not effective, is expensive to families, interferes unnecessarily with the education of well children, and 5th. It is not necessary because the spread of the disease may be prevented in other, and—in the opinion of your Committee—better ways.

MORTALITY OF MEASLES AS DEPENDENT ON SURROUNDINGS.

If the records of our various health boards prove anything they show, that the over-crowded districts, districts where houses are built mainly on

*British Med. Jour., Dec. 20, 1879.

†Medical and Surgical History of the War.

“filled-in ground” (the fillings often consisting largely of garbage and excrement), where district filth is a consequence of much personal filth, where fresh air, wholesome food, proper clothing, etc., are rarities, are the hot-beds for the spread of contagious and infectious diseases. In some, one year a “healthy ward” may have the largest mortality from a certain contagious disease, but taking a number of years together the over-crowded wards are ahead. Why? Because of filth and personal carelessness as to conditions of health, about which we know much, and secondly, because of meteorological conditions, about which we know but little.

PERIOD OF INCUBATION.

Heberden,	(p. 259)	gives the period of incubation as	13 days.
Eberle,	(Vol. I., p. 412)	“ “ “ “	5 to 7 “
Gregory,	(p. 228)	“ “ “ “	8 to 14 “
J. Lewis Smith,	(p. 160)	“ “ “ “	10 to 14 “
Meigs & Pepper,	(p. 765)	“ “ “ “	5 to 20 “
Tanner,	(p. 158)	“ “ “ “	10 to 14 “
Hartshorne,	(p. 343)	“ “ “ “	10 to 15 “
Sir T. Watson,	(p. 1171)	“ “ “ “	10 to 14 “
Flint,	(p. 996)	“ “ “ “	6 to 10 “

Flint further says that various authors give as wide a margin as from 1 to 30 days.

Dr. J. Sullivan, of this city, in 1,000 cases, found it to be 10 to 14—mean number of days, 12. Prof. Armor believes the period is variable, but is about *seven* days.

Dr. Edward Petzold, of New York, has seen 3,000 cases of measles, and found the period vary from 1 to 3 days.

Of 39 cases occurring at the Home for Destitute Children in this city, in January and February, 1880, the time of incubation was satisfactorily determined in 35 cases. The first four exposed in Sunday-school, together with two children in two private families exposed at the same time, showed symptoms of measles on the 13th and 14th days. These four were promptly and completely isolated on January 11th. No more cases appeared until—

Jan. 21.....10 days.....4 cases.	Jan. 24.....13 days.....10 cases.
Jan. 22.....11 days.....3 “	Jan. 26.....15 days.....6 “
Jan. 23.....12 days.....6 “	Jan. 27.....16 days.....2 “

The greatest number were attacked on the 13th day, and the average time of all the cases was 13 days.

In addition to individual peculiarities as to the period of incubation,

there have been individual peculiarities especially noticed this year, as to the method of invasion. In a number of children exposed to measles, after 10 to 16 days daily intermittent fever supervened, with a temperature of from 102 to 104° F., at night, in some instances attended by delirium. This fever came only in the afternoons, the children feeling well in the mornings. It lasted from 3 to 6 days, and was then followed by an eruption, or after the use of quinine there would be an interval of quiet, followed by a second attack of fever, and then the eruption of measles. At times the eruption is little or nothing; there may be a doubt as to measles. Shall the well children be kept from school, where there is a doubt? In our experience this year there have been many cases without cough, or conjunctivitis, or so little as not to be attended with photophobia.

Can we by isolation of affected children in a family, with any certainty, prevent the spread of measles to the well children? We believe not. During 1879, in the Home for Destitute Children, there twice occurred single cases of scarlatina. The children attacked being promptly isolated, the disease spread no farther. The same course was pursued in the case of measles; the eruption not appearing on any of the children first attacked until from 1 to 3 days after their removal to the hospital room, and yet 31 children had measles, from these first isolated cases.

QUARANTINE.

In regard to quarantine. Some physicians believe it to be advisable, as Dr. Jewett puts it, "on the general principle of stifling contagious diseases;" or, as Dr. H. N. Read writes, "to lessen sickness in the community." Professor J. Lewis Smith believes "that all diseases of childhood are contagious by the breath or exhalations, and therefore cases of measles should be quarantined." While some physicians believe with Dr. A. C. Post, of New York, that "it is best that children should have measles early." Others again agree with Dr. C. L. Mitchell, "that quarantine should be enforced for the benefit of infants and strumous children, but not for other children, that they may get it before 10 years of age." A fourth class of physicians are represented by Professor Armor, when he says, "I doubt the value of quarantine, for the reason that the contagion is developed at a *very early stage*, on the second, and possibly even the first day, a period so early that the nature of the difficulty cannot possibly be known. Hence the difficulty of preventing its spread." Bristow, in the very passage embodied in Dr. Raymond's paper, says: "Its contagiousness is fully developed at a very early stage, being at its height on the second, if not on the first day of invasion, and consequently before the specific nature of the attack is revealed. Hence the great

difficulty, if not the impossibility, of effectually preventing its spread in households and in schools." We need not quote further, as there seems to be no contrary evidence as to the contagiousness of measles in its early stage. There is no evidence of contagion in the desquamative stage. In fact, since inunction has become a common practice, the discovery of any desquamation is a rarity. Then, as the fever subsides, children generally recover so rapidly that desquamation is not watched for. Whereas, in scarlatina it is accepted as a fact that the exanthem is most contagious in the eruptive and desquamative stages.

Is measles conveyed by fomites? While it is true that most writers state that it is, yet your Committee cannot find accounts of actual cases, detailed as cases of diphtheria and scarlatina are, even in medical journals and periodicals. The transmission by letters and pictures, as spoken of in Ziemssen, does not seem probable. The conveyance by clothing that has been in the air since the exposure to measles, is not in accord with Mayr's observations. He denies any *excessive tenacity* of the contagion, since he could not ascertain that the disease was communicated by the garments and underclothing of the patients brought into the hospital with measles, when such clothes, after being aired, were made use of by other children in the building.* Airing seems to be a valuable preventive of contagion. Dr. Petzold, after his experience as physician to St. George's Hospital at Leipsic and the Orphan Asylum attached, and also as physician to St. Joseph's Orphan Asylum in New York, in both of which institutions there were many cases of measles, says: "Children in confinement are more apt to get measles than those living most in the open air. I believe the disease is an epidemic."

If the disease can be conveyed by fomites, then your Committee must believe that physicians are largely responsible for its spread, and especially are the medical inspectors of our health boards. The laity, during the last ten years, have been instructed in health matters, and they can rightly insist upon the physician, who has been exposed to measles, exercising as much or more care in attendance upon their children for other disease than measles, as they can if the doctor had attended puerperal metritis, and then expects to attend a confinement case.

If the attempt is made to enforce quarantine, will it be effective? Not in our opinion. There are many cases of measles that the doctors do not see. There will be more, if quarantine is enforced. Again, what control can there be over children kept from day schools, but who go to Sunday-school, play with children upon the streets, or ride in the cars with impunity. The people will not agree with Dr. Leaming, "to pro-

* Ziemssen, Vol. II., p. 37.

fect from exposure for four weeks after the beginning of subsidence of the exanthem," though they may agree that measles is not always a trivial disease, and the people have rights. In the words of a correspondent, "We do not think the disease of sufficient gravity to make no possible escape from it a sufficient compensation for the disturbance and trouble necessitated by an efficient quarantine." It seems eminently unfair to keep all the school-going children in a house at home, though all of them may have had measles, simply because one child is sick with measles and there is a bare possibility of some contagion finding its way down stairs, it may be from a fourth floor, and even when the family on the top floor is quarantined.

Children staying out of school for weeks lose their standing in school. Parents may have had to pay the school-bills, without receiving tuition in return ; such results are demoralizing. The case is more serious where teachers are concerned. In one instance, a teacher boarding in a family in which there was a child sick with measles, was compelled to leave school lest the disease should be carried to others in her clothing. Two teachers in the Polytechnic Institute were compelled to change their boarding-place or be excluded from school.

Is such action warranted by what we *know* of measles ? We think not. If such action is taken in regard to measles, about which at present we know little, why may not pertussis, and mumps even, fall into quarantine line ?

How, then, can the spread of measles be retarded ? By informing the people that measles is not always a trivial disease, that its spread depends largely upon a neglect of hygienic laws. Point out the proper methods of living by means of health tracts.

Have maps for reference prepared as insurance maps are, but for health purposes. As Inspector Raymond truly said :* "The great object of sanitary work being to diminish the death rate of a community, it seems absolutely essential that some method of registration should be kept in every health office, by which each house may be charged with the deaths that take place within its walls. From such a record can be learned at a glance where sanitary work is needed and much purposeless inspection saved." Will the present Sanitary Superintendent see to it that such valuable maps are on file ? Why should the same regulations be enforced with the same rigidity in the case of careful laymen and physicians as in the case of the careless or ignorant ? Would not the subject of contagious diseases be more satisfactorily studied and attended to by special inspectors of contagious diseases ? Could not the meteor-

* P. 52, Rep. Bd. of H., '73 to '75.

logical and other conditions which give rise to contagious diseases be better studied, and the results given to medical men for their guidance in a more tangible form than at present? In short, it is the opinion of your Committee, in relation to the spread of measles everywhere, as Dr. Day, the Sanitary Superintendent of the New York Health Board, said* in reference to contagious diseases in general: "The question of preventing the introduction and spread of contagious diseases in the public schools depends more upon the discrimination and good sense of the people than upon anything that can be effected by legislation. The influence of the physician could be made most powerful in that direction, and he would be able to control certain matters which it would be impossible to reach by legal enactments."

Our boards of health have been efficient, and the efficiency has been largely due to the sanitary superintendents; but in the future physicians will have a right to insist upon their being *depots of reliable information*, if more zeal and watchfulness are to be required from medical men. The laity, too, will hold doctors strictly responsible for the part *they* take in the spread of contagious diseases.

JEROME WALKER, M.D.,
BENJ. EDSON, M.D.,
N. B. SIZER, M.D.,
Committee.

DISCUSSION.

DR. RAYMOND said that the Committee deserved a great deal of credit for the amount of research given the subject, and for the conclusions at which they have arrived. The Society had been fortunate in having selected a Committee who were not satisfied with making a single annual report at the end of the year, but who reported upon various subjects from time to time during the year as occasion seemed to demand, and upon such matters as were at the time of the report engaging the special attention of the profession.

DR. WALKER commences his paper, continued the speaker, by stating that the Committee had been unable to find very much material out of which to construct or very many references germane to the subject. In that respect, Dr. Walker had proved himself to be entirely wrong. Although he had kept no running tally of the references made, or of the authorities quoted, yet Dr. Raymond felt sure that there were at least fifty or more.

Other than this, there were some points in the report of the Committee to which he must take exception. For instance; The Committee seem to question the possibility of communicating measles by fomites. They base their conclusion upon the idea that there have been no well-observed facts. He was sure that some of the authorities from whom quotations had been made had mentioned cases sufficiently well-authenticated where measles were thus conveyed; and the only answer that the Committee appear to be able to give is, that this does not seem probable. It would appear that

* Med. Rec., Jan. 20, '77, p. 44.

these authorities, Aitken, Niemeyer and others are entitled to the greatest credence. Unless we have something to the contrary, we should accept their testimony. It seems that there is just as much evidence to support the view that measles is conveyed by fomites as there is to support any other facts in medicine. Should the new members of this Society be questioned, how many of them of their own experience can point to a single case of small-pox which has been conveyed by clothing, or any other means, for that matter; or of scarlatina? Just as in the case with small-pox and scarlatina, so with measles. Yet does any one doubt that small-pox and scarlet fever can be conveyed by clothing or other articles? Aitken, Flint, Bristow, Thomas, in Ziemmsen's Encyclopedia, all believe in the contagious qualities of fomites. In fact, he knew of no author who held the contrary opinion. An instance was mentioned in the paper read at the last meeting, of an intelligent physician who came from a case of measles, took his child upon his knee, then put it down, and within a few days the child was attacked with the disease. You may say, how do you know a child has been exposed? You don't know. So with the authorities quoted. But they were competent observers. Such men as Thomas, who was engaged to write upon this disease in Ziemmsen's Encyclopedia, are not to be overlooked. We must accept their statements. But the point is established here beyond a possibility of a doubt, namely: that measles may be and is conveyed from one to another by fomites.

He had no doubt but that the Committee had some authority for their statement regarding the clothing of physicians as a means of conveying measles, and they seem to think that the inspectors of the Board of Health are more responsible in this regard than any other men. If they visited cases of measles that might be so; but inasmuch as no single case of this disease is visited by a sanitary inspector, their strictures, as a matter of course, fall to the ground.

In regard to the question of quarantine, the Committee have shown that measles is "not a trivial disease." It is one which, they report, is followed by almost every other in the catalogue of diseases. Its complications are unlimited. When the report was being read, he had time to think what diseases there were known to medical science which were omitted from the list of sequelæ, as given by the Committee, but he had thought in vain. The Committee say that it may be followed by eye and ear trouble; by pneumonia in its various forms; by diseases of the brain, as meningitis and tubercular meningitis; by diseases of the nervous system generally; by all forms of disease "that human flesh is heir to;" and YET the Committee do not believe that the slight interference with the education of the child necessary is worth while, or that the disease is of sufficient severity to justify the expense to the family of a few days' absence from school, at home! If this report were written by three bachelors he would not be surprised at the sentiments expressed therein, but having among its members some, at least, who were fathers, his surprise was great indeed. What expense to the family is it to have a child remain at home for a few days, that it may escape the fearful ravages of this disease, as compared with the expense of sickness, and, probably, death, with the long list of funeral and mourning expenses, to say nothing of the trouble to the household and friends, and to all concerned? Besides, what detriment will a few days' absence from school be to a child, provided he escape the sequelæ of measles, its possible blindness, deafness, maiming, and the innumerable other evils of which the Committee speak? It seems that this is a part of the subject which has been overlooked by the learned Committee. How shall we judge of the severity of the disease? Are we to look at the mortality reports and judge by the results therein recorded? Where do you find a record of the eyes and ears lost, of diseased lungs? Are these hings

not to be considered in the question—are they not to have any weight at all? Is the expense to the family of various kinds not to be taken into account? It seemed to him if a single life could be saved, the expense of a thorough quarantine by the Board of Health ought to be enforced. There may be other means than keeping the afflicted child from school, but because we cannot do *everything*, are we to stop and do *nothing*?

In regard to isolation in hospitals, any one who has had anything to do with hospitals knows that there is no such thing as isolation in hospitals, especially in eleemosynary institutions. There is no such thing as isolation in such places. The same doctors and nurses go from one room to another—from a case of pneumonia to a child having scarlatina, then to a case of measles, and thus the disease spreads. Measles spreads, and scarlatina does not so rapidly, because measles is a much more communicable disease. It is much less controllable and spreads more rapidly; and, therefore, goes through a house or institution where scarlatina would not.

In regard to second attacks. If there is one thing which is brought out by this discussion more than another, it is that second attacks of measles are by no means so rare a thing as we are led to believe by the books. Could any physician in the room mention twelve second attacks of small-pox or scarlatina occurring in his own experience? He could cite two physicians of the Society who have had under their own observation twelve second attacks of measles. There are physicians, also members of this Society, who had attended third attacks—although third attacks are by no means so common. They themselves say that we should treat the disease exactly as if every individual were subject to another attack. Especially should this be done if the disease can be conveyed by fomites. In regard to pertussis, that is true; and yet who is satisfied that pertussis can be conveyed by the clothing? He thought there is no more valuable procedure than isolation in a case of a child attacked with whooping-cough. That is a measure which should always be thoroughly carried out. No child not thoroughly cured should be allowed to go to school.

And there never will be a time when *all* cases of contagious disease are reported. There are a great many cases of small-pox during the existence of epidemics which are not reported; so with scarlet fever; and yet shall we not quarantine these diseases? Is there not to be some sanitary regulation to govern the spreading of these diseases, simply because every case is not reported?

It is objected that quarantine should not be established, because teachers could not be allowed to attend schools, and they will thus be kept away from their regular and necessary duties. Is there any member of the Society who, knowing that a teacher had come from a case of contagious disease, would like to have his child in the room with that teacher? Physicians are familiar with these diseases; they are well acquainted with all their horrible sequelæ; and in the light of this knowledge he asked this question: Would they like their child in the room with such a person for four or five hours? Would they like such a person to come into their family of little children? He for one was sure he would not. That is the way to look at these questions. *Take them right home to ourselves.* The argument then has a force and weight which it cannot have under other circumstances.

The Committee's report brings out a great many facts which are exceedingly valuable; but it should not be forgotten that men are in the habit of looking upon these diseases too much with reference to the mortality tables. If we find a disease killing thirty or forty children a year, we look upon it as a comparatively mild disease; and yet measles, in the first three months of the year, killed more than during the whole of 1879. Measles is *peculiarly* liable to change in different epidemics. Because we read in books that "measles is a mild disease," we are not to infer that measles in

Brooklyn this year is a mild disease. Look at the records read by the reader of the report. This epidemic may be such an epidemic! If it is found that in the three months of this year it has killed more than in the twelve months of the preceding year, we must give the matter some attention. He could not agree with the gentlemen that this matter of quarantine should be left to the good sense of the people. If there is anything that the people have not got, it is this so-called "good sense." He had known people who insisted that children ought not to be sent home from private schools, not only on account of measles, but of scarlet fever. So he had known of people who cared no more for a case of small-pox in the upper rooms of a tenement than they would for a case of measles. Tell them of their danger, and they would laugh at it. People have no "good sense" in these matters. You cannot trust them; it is worth nothing!

DR. WALKER arose merely to state that this report had been brought out after months of labor. It had taken considerable time—more, probably, than he would ever bring to bear upon any other paper; and the Committee had carefully considered most of these matters. They had not said anything about the *treatment* of measles and some other points, but they had taken the other horn of the dilemma; they had raked up all they could bearing upon the mortality of this disease; and they have come to the conclusion that the disease depends upon certain neglected conditions which can be controlled by certain sanitary regulations. He did not think the disease could be conveyed by fomites; neither did he think that any gentleman of this Society thought so.

DR. RAYMOND wished to refer to just one point further, and that is in regard to the prevalence of the disease in districts in which there is a squalid population and unsanitary surroundings.

In the 12th Ward, with a population of 17,576, there were, at the time the last table was made, 24 cases and 4 deaths.

DR. WALKER asked what time this table covered?

DR. RAYMOND replied that it covers only this present year.

DR. WALKER said his tables covered a longer period.

DR. RAYMOND continued: In the 7th Ward, with a population of 26,494, there were 200 cases and 6 deaths; the 13th Ward, population 19,649, there were 20 cases and 2 deaths; 14th Ward, population 23,926, 26 cases and 3 deaths. Now the 12th Ward is a tenement-house ward, the ground is filled in and contains much filthy matter, and because of the presence of all those elements which are supposed to be conducive to unhealthiness, it has always been an enigma. So far as the measles is concerned, it stands the lowest; while wards having better sanitary surroundings stand higher.

DR. HART spoke of the probability of many adults falling victims to the disease in epidemics.

DR. SHERWELL referred to the many errors of diagnosis in determining whether they were second attacks or not. For instance, he had had opportunities of seeing gentlemen at different times, whose mothers stated that they had had in earlier years the usual attack of measles. Upon close examination, the eruption for which the doctor was called to attend was an eruption caused by the use of copaiba. So there is a form of roseola which simulates measles. He remembered a case at Packer Institute of what is known as a bastard form of measles. So there are forms of syphilis and roseola which simulates the disease under consideration. It is sufficiently like measles to be mistaken for it. There is a fever of the mucus membrane, coryza, more or less, etc., etc. He did not think these second and third attacks were quite as common as

Dr. Raymond supposed, nor did he think these secondary and tertiary attacks were quite so frequent as the report would seem to indicate.

DR. RAYMOND was aware that it is doubted by some medical men whether second attacks do occur. There are two sources of error in determining the question: First, that the first attack was not seen by the attending physician, and secondly, that the physician who examined the case might make a mistake.

The doctor then read the testimony of a number of competent observers in relation to second attacks of measles, as follows:

DR. C. R. MCCLELLAN says: "More than once, say twelve times."

DR. ALEX. J. C. SKENE: "I have seen two (2) children who were attacked six (6) weeks after recovering from the first. The second attack was rather milder than the first."

DR. SAMUEL G. ARMOR: "I have three cases in mind at present in which the patients had repetition of the disease within less than three months after the first attack. * * * The second attack in one of these three cases was very severe; two of them were light."

DR. S. C. GRIGGS: "I had one patient who had measles three times at intervals of one year. I have never had more than two recurrences of measles."

DR. ARTHUR MATHEWSON: "I had measles very mildly at 10 or 12 years of age, and again mildly, when 23 years of age."

DR. WM. GILFILLAN: "In a practice of twenty years I have seen measles twice in two cases. Once in my daughter and once in my sister-in-law."

DR. JAMES WATT: "Out of about 30 cases 5 or 6 second attacks; 3 in one family."

DR. HOPKINS: "One patient twice, at an interval of two weeks."

DR. F. H. STEWART: "The number of cases I do not recollect. I have had several where it had occurred twice."

And so, he continued, he might go through many names of the members of this Society: Dr. Burge, Dr. Otterson and Dr. Dodge, with evidence to the same effect. If there is any fact which rests upon ample and sufficient medical evidence, it is that measles does occur a second time. In the practice of some physicians it has occurred twice in as many as twelve cases.

DR. SHERWELL did not deny that such things do occur; he stated that frequently other affections of the skin may be mistaken for measles, and this was a source of error in statistics.

FURTHER CONTRIBUTION TO THE STUDY OF FRACTURES OF THE INFERIOR EXTREMITY OF THE RADIUS. DIFFERENTIATION OF LONGITUDINAL AND TRANSVERSE FRACTURES AND THE CAUSES WHICH PRODUCE THEM.

BY L. S. PILCHER, M.D.

It is now two years since I had the privilege of presenting to this Society a memoir upon fractures of the radius near the wrist joint, in which I demonstrated, by a method of my own, that transverse fractures of the lower extremity of the radius were produced by a force of avulsion, conveyed through the anterior carpo-radial ligament, a truth which, though it had been fully demonstrated eighteen years previously by Le Comte in France, had been generally lost sight of in this country. I was able to add to what had been previously described as to the pathology of these injuries two new facts, viz. : the action of the internal or carpo-ulnar fasciculus of the anterior ligament of the wrist in locking the forearm in a state of extreme supination, as long as the backward displacement of the lower fragment of the radius, if at all great, remains unreduced; and secondly, the frequent existence of a tense pseudo-ligament formed by the stripping up of the dorsal periosteum from the upper fragment, reinforced by the dorsal annular ligament and the sheaths of the extensor tendons, which having remained untorn, and having been forcibly put upon the stretch at the moment of the backward displacement of the lower fragment, tends to finally hold the fragment in any position of entanglement into which it may have come. In this memoir I further called particular attention to the preponderance of sprain as an indication for treatment, and having discussed the special conditions presented by sprains of the wrist joint, and having shown that fractures of the inferior extremity of the radius *per se* never entailed permanent disability, but that in the results of the sprain were to be found the fruitful source of long continued, and sometimes permanent impairment, of the function of the joint, and having shown that sufficient support for the satisfactory retention of the fragments after their reduction could be afforded without the use of splints, I advocated strongly the disuse of splints in the treatment of this injury, and the use of a simple retentive bandage with massage.

These views received from the profession in general a much more favorable notice than I would have dared to expect, and from many quarters I have received assurances from surgeons and surgical teachers, of their endorsement of the views of that memoir.

In addition, my friend and colleague, Prof. Jarvis S. Wight, has done me the honor of carefully considering my memoir and of commenting upon it at length in an erudite and logical paper which appeared in the *Medical and Surgical Reporter*, of Philadelphia, in November, 1878. The conclusions which he arrived at, as inferred from his statements, are that the memoir in question does not display a full knowledge of the differing facts of the accidents of which it professed to treat, that it is disingenuous in its statements of the results of the experiments detailed, that its inferences are not sustained by the facts adduced, that its statements of anatomical conditions and of functions are incorrect, that its conclusions are erroneous, and that its doctrines are dangerous to the reputation and purses of those surgeons who may adopt them.

The high respect which I have for the professional attainments of my commentator cause any views which he should express to command at once my attention. Moreover, the authority which his position as a professed teacher of surgery gives to his words would make it impossible for me to dismiss them lightly.

Still more, the high principles, with the announcement of which he prefaces his paper, find a quick response in my own mind, and I adopt his words as my own, *verbatim et literatim*:

“The object in writing this paper is to establish scientific truth, to lay a foundation for correct practice, and to aid in defending the surgical profession against unjust suits at law for malpractice. It is safe to say that the truth is better than any man; but if the false has been made to seem to be true, and thereby one reputable surgeon has been unjustly ruined in reputation and prospect, it is also safe to say that the responsibility of his ruin rests with those who have, perhaps without intention, made the false seem true.”

My friend evidently felt that it was an unpleasant duty which he had to perform, for he proceeds to say that, by the combination of various circumstances, it is imposed on him as a simple and imperative duty, a duty which is fully discharged only when he has lectured the three eldest and most renowned surgical teachers of New York City, singling them out by name, upon their dangerous course in endorsing, in any degree, these new doctrines!

I have felt that it was incumbent upon me to re-examine this whole question with whatever of candor and capacity I could command, and either to candidly acknowledge my errors, if such I had fallen into, and to recant from any dangerous doctrines, or if, on the other hand, further examination and experience should more firmly convince me of the correctness of my former positions, to do my best to convert my deceived friend from the error of his ways, and to place him on that basis of scientific truth he holds so dear. I have not found the year and a half which has elapsed since the publication of Prof. Wight's article any too

long to accomplish the task which I had undertaken, while it has been long enough, I hope, to enable me to bring to this present discussion a spirit of calm scientific inquiry. The Professor, after the settling of some preliminary measurements and definitions, which indeed impress one with the care and research which he brings to his task, opens the discussion of the relations of the hand to the radius in forcible extension, by comparing the hand to a lever of the first order, the radius being the fulcrum upon which it rests. The hand constitutes the long arm, the arm to which the power is to be applied, and the short arm is *that portion of the carpus between the insertion of the flexor muscles* and the point of greatest pressure on the inferior extremity of the radius, the length of which, he states, is apparently one inch.

Here at once I am bewildered, for in all the wrists I have ever examined, I have never seen one yet in which, in extreme extension, any part of the carpus projected one inch in front of the point of greatest pressure against the articular surface of the radius. Then again there is but one little flexor muscle—the flexor carpi-ulnaris—attached to any part of the carpus, either before or behind the point of greatest pressure against the radius, while the great mass of flexor tendons sweep over the trochlear surface afforded by the carpus, and pass forward to their insertions into metacarpus and phalanges.

The anatomy of my critic is certainly at fault at the outset. Perhaps his mechanics will be better. Proceeding with his original hypothesis of the lever, he states that the ratio of power and weight force, as determined by the relative lengths of the two arms of the lever, will be as 1:6. And then he says: Suppose 100 pounds of extension be applied to the power end of the hand lever, there will be a pressure of 700 pounds on the lower extremity of the radius. Now, this hypothesis is entirely untenable, for the very essence of it is that six-sevenths of this weight is muscular contraction, and it goes without saying that the contraction of the muscles will have been overcome long before the hypothetical amount is reached, and the terrible results of a pressure of 700 pounds on the lower extremity of the radius will have been averted. But even here he has persisted in assuming that the hand lever is a lever of the first order, while really it is a mixed lever, viewed from the standpoint of the flexor muscles, being partly of the second and partly of the third order, and partaking not at all of the first order. Incorrect in anatomy, incorrect in mechanics; and these in his premises; there is a possibility that his ultimate conclusions may be incorrect also.

A series of eleven cases are next marshaled in order—two from his own experience, and seven collated from various text books. The two first are entirely irrelevant—one of them does not pretend to be a fracture

of the kind under discussion, and the other presents no proof of ever having been any kind of a fracture. The seven collated ones illustrate very well the differing conditions which fractures of the lower extremity of the radius may present. Later follows a description of the experiments of Nelaton, and their repetition by Dr. Daniel Ayres, with the positive assertion "so that M. Nelaton made a Colles fracture in the dead forearm, by compressing the base of the radius between two counter-forces; and this on the authority of Malgaigne."

This is his *pièce de résistance*, upon which, after all, it is apparent that he bases chiefly his ultimate conclusions. The doctor does not seem to be aware that the real character of the experiments made by Nelaton were quite different from what both that distinguished surgeon and his contemporary, Malgaigne, supposed them to have been; and that 18 years ago the error of these gentlemen was demonstrated, and their experiments shown to have proved an entirely different thing. Finally the doctor makes the issue squarely on the assertion contained in my original memoir, that the theory that fracture of the lower extremity of the radius happens because the bone, having been caught between two forces, gives way at its weakest part, is not sustained by the facts of the case, and as the conclusion to his own reasoning formulates the following theorem:

"A fracture of the base of the radius is generally caused by the reaction of the resisting surface on which the palm of the hand strikes at the time of the fall, the carpus being driven or pressed against the base of the radius."

I accept the challenge, as thrown out by the Professor, as to the facts of the case, and as a basis for argument repeat, with the addition only of the word transverse, as modifying the word fracture, my original theorem:

Transverse fracture of the radius near the wrist joint results almost invariably from falls, the force of which is received upon the palm of the hand.

The theory usually adduced in explanation of the injury resulting is that the radius, having been caught between two counter-forces, has given way at its weakest point, about one-half inch from its lower extremity.

An examination of the facts of the case does not sustain this theory.

For purposes of study these facts may be classified as follows :

First, those pertaining to the place and character of the fracture.

Second, to the anatomical structure and relations of the fractured part.

Third, to the character of the violence which has been known to determine these fractures.

PLACE AND CHARACTER OF THE FRACTURE.

In studying the place and character of fractures of the lower extremity of the radius, I have examined eleven specimens of recent fracture contained in the Museum of the New York Hospital, and one in the collec-

tion of Professor F. H. Hamilton, an analysis and study of which have not heretofore been made.

In their examination I note : First, that ten of the twelve present lines of fracture running both transverse to and parallel with the long axis of the bone ; in the greater number these vertical lines of fracture involve only the lower fragment, which is comminuted as the result ; in some these vertical lines extend as lines of fissure up into the shaft itself.

Second, that impaction of the upper in the lower is present in only one specimen—No. 123—and in this so slightly, that little force would be necessary to overcome it.

Third, that a great variation exists in the amount of the articular extremity which has been separated from the shaft.

In one specimen—No. 125—the lower fragment is composed of the tip only of the styloid process of the radius ; in a second—No. 127—the whole styloid process is broken off, the line of fracture running obliquely from its base downward to the bisecting ridge of the articular surface ; in these two specimens the dorsal lip of the articular surface of the radius has been crushed off, and a fracture of the scaphoid bone also sustained ; in a third specimen—No. 122—a yet larger portion of the articular surface is included with the fragment, the line of fracture running from the base of the styloid process to the middle of the quadrilateral articular surface for the semi-lunar bone ; in a fourth—No. 121—the whole of the carpal articular surface of the radius is carried away with the lower fragment, by a line of fracture running very obliquely from the base of the styloid process to the point of junction of the carpal, and the semi-lunar articular surfaces.

In other specimens—Nos. 123, 124, 129, 125^a, 120 of the New York Hospital collection, and the specimen belonging to Professor Hamilton—the lower fragment includes the whole of the inferior extremity of the bone, which is separated from the shaft by a line of fracture running nearly transversely at points varying from one-quarter inch to one inch above the articular margin in front, with an antero-posterior obliquity, so that on the dorsal aspect of the radius the line of fracture is somewhat higher above the articular margin than was the case upon the palmar aspect ; in the specimen—No. 124^a—the line of fracture is lowest upon the dorsal aspect of the bone where it runs along the epiphysial line about $\frac{3}{8}$ in. above the articular edge, thence it runs obliquely upwards as it approaches the palmar aspect, where it terminates about one inch above the articular edge.

In the remaining specimen—No. 128—there is simply a longitudinal fracture running upward from the articular surface, near the ulnar margin, by which a bit of the radius containing the ulnar articular facet is split off. This was accompanied by a fracture of the scaphoid bone.

It is to be noted that all these were instances of injuries, where a great degree of violence was sustained, and other injuries of fatal character received.

As to the place and character of fractures of the lower extremity of the radius, I conclude therefore from these specimens that the fractures which do occur are of two distinct general classes—transverse and longitudinal; that in transverse fractures the amount of the lower extremity separated from the shaft is extremely variable; that the line of transverse fracture is rarely above one inch above the articular margin; that it may approach it very closely, may involve it at any point, and may include only a portion of the styloid process; that longitudinal fracture may coexist with transverse fracture, or may be present as the only lesion.

The recognition of the two distinct directions of the line of fissure which these fractures may present, carries with it the recognition of the necessity of the bone having been subjected to forces acting in two distinct directions upon the bone at the time of these injuries, one acting in the line of the axis of the bone tending to split it, a force of cleavage, one acting transversely to the axis of the bone, a cross-breaking strain.

ANATOMICAL STRUCTURE AND RELATIONS.

A brief enumeration of those prominent points connected with the structure and relations of the lower extremity of the radius which may be of importance in tracing the connection between the place and character of the fracture and the violence which has been its determining cause is here in place. The radius at its lower end becomes expanded in every direction to adapt it for the carpal articulation. This expansion is greatest anteriorly and externally, and results in the development of the massive styloid process and the prominent anterior lip. The thick and dense compact tissue which forms the walls of the shaft above becomes gradually thinner as it enters the expanded lower end, which is composed of cancellous tissue enclosed within a very thin compact shell. In front the thick compact wall descends with but little diminution in its thickness to within half an inch of the articular margin, and then shades off abruptly; behind, the compact wall shades off very gradually and terminates nearly half an inch higher up.

Upon inspection of a longitudinal section of the radius it will be seen that the cancellous tissue of the lower extremity forms a distinct wedge which is received between the diverging thick external compact laminæ of the shaft above. The projection of the anterior lip in front of the plane of the anterior surface of the shaft is equal to one-half the diameter of the shaft. The posterior lip is less prominent, descends slightly lower than the anterior, and, notwithstanding the posterior expansion of

the shaft immediately above it, is itself on a plane slightly anterior to the plane of the posterior surface of the unexpanded shaft. The lower half or three-quarters of an inch of the radius makes with the shaft a slight angle, being bent forwards so as to bring the greater part of the articular surface in front of the axis of the shaft and to give an anterior obliquity to its surface.

The articular surface is slightly concave both from before backwards and from side to side. The first row of the carpus presents a sharply convex surface both from before backwards and from side to side, of which surface but a small portion is in contact with the radial articular surface at any one time. A considerable degree of antero-posterior motion is possible in both the mid-carpal and the radio-carpal articulations. This motion is most extensive in the latter articulation. By the sum of the two the hand can be flexed and extended nearly to a right angle with the forearm without bringing undue tension upon the ligaments of these joints.

The anterior radio-carpal ligament unites the radius and carpus in front, being inserted above into the projecting anterior lip of the radius. The most dense and strong portion of this anterior ligament is a band which arises from the ridge just above the styloid process, and from the uneven depression between that ridge and the margin of the articular surface for the scaphoid bone, and passing obliquely downwards and inwards, is inserted into the semi-lunar, magnum and unciform bones; blending with the outer edge of this band is the external lateral ligament, which, arising from the extremity and forepart of the styloid process of the radius, passes to the inner and forepart of the scaphoid and the adjacent edge of the os magnum, so that its fibres pass to the forepart rather than the side of the joint, reinforce the anterior ligament and combine with it in limiting extension. That portion of the anterior ligament which arises from the eminence that surmounts the articular surface for the semi-lunar bone passes downward less obliquely and is attached to the magnum, unciform and cuneiform bones. From the anterior margin of the head of the ulna, from the styloid process of the ulna, and from the inner border of the ulna at the base of its styloid process, a third strong band arises, which passes obliquely downwards and outwards and is inserted into the forepart of the cuneiform and the adjacent edges of the unciform and semi-lunar bones; its inner edge blends with the internal lateral ligament, which also contributes to limit extension. The anterior ligament is further reinforced and its ability to limit extension increased by the flexor tendons of the fingers which lie upon it, together with their investing sheaths.

The posterior radio-carpal ligament permits a degree of flexion

nearly equal to that of extension. Comparatively weak, it is powerfully reinforced by the extensor tendons, their sheaths and the posterior annular ligament.

The carpal and the metacarpal bones are joined together with such firmness that but slight motion is permitted between them. In the movements of extension and flexion at the wrist they act virtually as one bone.

THE CHARACTER OF THE VIOLENCE WHICH HAS BEEN KNOWN TO DETERMINE
THESE FRACTURES.

The most frequent cause of fracture of the lower extremity of the radius is universally conceded to be a fall, the force of which has been received upon the palm of the hand. In many cases the fracture has resulted from falls, attended with but little violence, as where a person, having slipped, has thrust out his hand to break the fall. Where a great degree of violence has been sustained, as in falls from a height, this particular lesion of the radius is usually found to be complicated with comminution and marked displacement of its lower fragment, and with stretching and more or less complete rupture of the radio and carpo-ulnar ligaments; the styloid process of the ulna may be broken off, and the head of the ulna caused to project strongly upon the antero-internal aspect of the wrist, and, in extreme cases, even may be forced through the skin.

Fractures of the lower extremity of the radius have been produced by simple forced extension of the hand.

Voillemier, in 1842, first called attention to this fact, and reported two cases exemplifying it. Occasional examples have been reported since; to their number I can add the following three, observed by myself:

CASE I.—Delia Moore; aged 30 years; residing at 626 Classon Ave., Brooklyn, N. Y. In the evening of December 30th, 1878, while standing on a chair, she slipped, and, falling, caught at a table to save herself; the fingers only of the outstretched hand reached the edge of the table and her hand was bent strongly backward; upon recovering herself, she found that her wrist was injured. She immediately applied to Dr. John Cooper, of 33 St. James Place, who recognized a fracture of the lower extremity of the radius, with displacement. He at once reduced it, but sent her to me for further treatment. Upon examination, shortly afterwards, I was able to elicit distinct crepitus and to confirm the diagnosis.

CASE II.—Daniel Maloy; aged 14 years; residing at 42 Front Street, Brooklyn, N. Y. September 19th, 1879, while endeavoring to prevent a boat from chafing against a dock, his right forearm was so caught between the boat and the dock that the boat impinging against his elbow, and his hand pressing against the dock, the hand was forced into extreme extension. Within an hour after the accident I saw him, and he was also examined by Drs. F. H. Stuart and Geo. R. Westbrook. His right wrist presented the characteristic silver-fork deformity of fracture of the lower extremity of the radius; the line of fracture on the outside was about three-fourths of an inch above the tip of the styloid process, and thence ran transversely about one-half

an inch above the articular surface ; the lower fragment was displaced backward, without marked rotation, and was entangled upon the posterior edge of the upper fragment, the anterior edge of which was distinctly felt projecting upon the palmar surface.

CASE III.—Bernard Ettinger ; aged 16 years ; residing at 249 Atlantic Ave., Brooklyn, N. Y. September 23d, 1879, while assisting in lowering a case of tobacco down a staircase, he was compelled to support for a moment the whole weight of the case upon his extended hands, in an attempt, as he stood below it, to prevent its slipping. Immediately he realized that his left wrist had been injured. I saw him on the following day and determined the presence of a fracture of the inferior extremity of the left radius ; the lower fragment was slightly displaced backwards ; the line of fracture upon the outside was apparently one inch above the tip of the styloid process of the radius, whence it ran transversely less than half an inch above the articular surface ; the relations of the ulna were unaffected. This case was also examined by Drs. F. H. Stuart and E. H. Bartley.

An additional case, unusually free from any possibility of the presence of any other force than that of over-extension, is narrated by Prof. MacLeod, of Glasgow, in the *British Medical Journal* of July 12th, 1879. p. 39. The case was this: "A young man contended with an older and stronger man in a test of strength by placing elbows on a table, interlocking fingers, and then pressing back against each other palm to palm. The hand of the young man became violently extended, until finally something gave way with development of sharp pain in the radius, with the well marked deformity characteristic of fracture of the inferior extremity of that bone."

Fractures of the lower extremity of the radius have resulted from falls upon the back of the hand. Hamilton relates an instance of the kind.

Gordon relates an instance in which a man, aged twenty, whilst leading a horse out of a stable, had his forearm compressed between the door frame and the side of the animal, the hand being flexed at the time. The radius was broken about an inch and a half above its lower end.

In fine, the character of the violence by which fractures of the lower extremity of the radius are produced is fourfold, viz. :

1. Falls upon the palm of the hand ; common.
2. Simple over-extension of the hand ; unfrequent.
3. Falls upon the back of the hand ; rare.
4. Simple over-flexion of the hand ; very rare.

CONCLUSIONS.

After this analysis of the facts involved in fractures of the lower extremity of the radius, we may, with greater facility and accuracy, hope to trace the sequence of the causes to which may be due the varying conditions which different fractures present.

What is the order of events which happen in falls upon the palm of the hand ?

First, the impact is sustained by the thenar and hypothenar eminences over the base of the metacarpal bones. In extreme extension the prominences of the trapezium and unciform bones became involved in the impact. That the fall should come upon the palm of the hand at all, it is necessary that the hand should be previously somewhat extended, in which movement the points where the impact is to be sustained are carried back of the axis of the radius.

The immediate effect of the impact is to increase this extension. The distal row of the carpus glides backward upon the proximal row, the latter glides forward beneath the articular surface of the radius until it is checked by the distended anterior ligament, and the radius rests upon its posterior surface. The flexor tendons also—if the muscles be in contraction—antagonize this extension and reinforce the anterior ligaments. However, if the flexor muscles have been sufficiently strongly contracted to resist the further tendency to extension at the moment of the impact, the hand and carpus will be driven *en masse* upwards, as the resultant of the forces of extension and flexion, and the narrow convex surface of the upper row of the carpus will impinge against the broad shallow concave articular surface of the radius. In the transmission of the force to this point it will have already been greatly decomposed. The elastic adipose, fibres and muscular cushions of the palm, and the two rows of carpal bones, cancellous in structure with their interposed cartilages, unite in repelling shock; whatever of strain has been put upon the flexor muscles is just so much force transmitted to the origin of the muscles and there expended; whatever of ligamentous strain is sustained, is just so much of the original force transmitted to the insertions of the ligaments and there expended; whatever of the force of the original impact which is not thus expended is finally received upon the articular surface of the radius. Here an admirable arrangement is found to break up and render harmless whatever of force yet remains; a buffer of cartilage upon which rests a cone of cancellous tissue which is grasped above by a cylinder of strong, thick compact walls. All force transmitted to the lower extremity, other than that of very great intensity, of the radius, is here finally decomposed and repelled. If, however, as the result of very great violence sustained, as in falls from a height, an impact of the carpus against the radius is occasioned more powerful than can be resisted by the mechanism described, what will be the result? The problem is a simple one. The carpus is a blunt wedge; driven against the concave articular surface of the radius with sufficient force it will split it longitudinally. There is no possibility of a cross-breaking strain being exerted, and if in any case transverse lines of fracture are found, they must have been occasioned by some other mechanism.

The description of the results of falls upon the palm of the hand applies equally well to those of falls upon the back of the hand, substituting the words flexion for extension, extensors for flexors, and noting that the line of force falls in front of the axis of the radius instead of behind it. As a matter of fact, however, falls upon the back of the hand are of very rare occurrence.

The existence of these longitudinal lines of fracture in certain cases of injury from falls upon the palm of the hand was first signaled by Dupuytren fifty years ago, who, never permitting an opportunity to gain exact knowledge by an autopsy, stated that where an autopsy had permitted an examination of a recent fracture of the lower extremity of the radius, he had often found the extremity shivered with radiating lines of fracture, as if it had been struck a blow with a hammer. (*La Lancette*, VI., No. 4; quoted by Voillemier, *Arch. Gen. de Med.*, 1842, LVIII., p. 263.) But Duypuytren made no attempt to differentiate the causes of this shivering from that of the transverse fracture; the injury as a whole was attributed to the direct transmission of shock to the radius through the carpus. Nelaton, in his *Éléments de Pathol. Chirurg.*, I., p. 740, treating on the pathology of fractures of the inferior extremity of the radius, bases his views on the results of certain experiments on the cadaver; in these, by striking forcibly upon the upper end of the bones of the forearm, held vertically while the palm of the hand was supported upon a firm surface, he produced transverse fractures of the lower extremity of the radius. He imagined that the backward bending of the midcarpal articulation was so free that the first row of the carpus was permitted to impinge against the ground, and that through this directly the shock had been transmitted to the radius above. Upon the cadaver he secured transverse fractures only. *Post hoc, propter hoc!* Strong in his assumption of the relation of the carpus to the radius, he announced the formal doctrine that transverse fracture of the radius just above the lower articular surface is the result simply of the fact that the bone, having been caught between two forces, the resistance of the ground on the one hand, and the weight of the body, increased by the velocity of the fall, on the other, has given way at its weakest point. The only possible effect of the direct crowding of the first row of the carpus upward against the expanded concave articular surface of the radius, if it had taken place, was entirely overlooked by Nelaton, and longitudinal fractures had no place in his pathology. The genius of Nelaton was sufficient to impress his views on his compeers for a whole generation. Malgaigne and Voillemier accepted it, though both acknowledged the occasional production of the fracture by the unaided force of over extension of the wrist. The surgeons of England and America have accepted it, and in great measure

do accept it to this day, and much ingenuity has been displayed in demonstrating that the usual point of fracture in the radius, where it commences to expand with thinning compact walls and abundant cancellous tissue, is the weakest part of the bone, while the necessary preliminary of explaining how the longitudinal force, assumed to be acting, was converted into a transverse strain has always been strangely overlooked.

A new fact, however, was added to our knowledge of these injuries by Dr. H. J. Bigelow, when, on January 11th, 1858, he presented to the Boston Society for Medical Improvement a radius, the lower articular surface of which presented a *star-shaped* crack, and corresponding fissures extending therefrom upward in the shaft for more than an inch without transverse fracture. Other injuries received at the same time had caused the patient's death. This was the second case of the kind which had been verified by autopsy in the experience of Dr. Bigelow.

Here were two cases of longitudinal fissure of the inferior extremity of the radius, uncomplicated by transverse fracture. By what mechanism had they been produced? If, as Dr. Bigelow thought, the bones of the wrist had acted as a wedge to spread the corresponding hollow of the articulating extremity of the radius in these cases, would not the tendency to produce the same effect have been present in all cases where the bones of the wrist were crowded up against that articular extremity? How, then, can transverse fractures of the bone be the result of a force which merely produces a violent impact of the carpus against the articular surface of the radius?

The conclusion is inevitable; there is no principle of mechanics, no peculiarity in the construction of the bone, no peculiarity of the dynamic conditions present which can alter it, that such an impact, as in these two cases, so in all, will, when sufficiently violent, produce longitudinal fracture, and that only.

In view of the special adaptation of the mechanism of the wrist joint to successfully resist force applied to the palm of the hand, it is evident that the original force must be one of great violence to produce longitudinal fracture. In injuries resulting from comparatively slight violence—which includes the great mass of recognized fractures of the lower extremity of the radius—longitudinal fracture cannot be present; but in falls from a height in which the weight of the body with great momentum comes upon the outstretched palm, the conditions are presented in which, if our reasoning be correct, longitudinal fracture may be possible. These are the cases likewise in which other severe injuries are likely to be present, which, by their fatal results, may make actual inspection of the fractured bone possible. These are they which find their way to hospital wards and dead houses. These formed the class of cases which Du-

puytren inspected, and which gave rise to his graphic description of fragments shivered and splintered as if struck by a hammer. To this class belong the twelve cases immediately under examination, in which it has already been recorded that ten present the articular extremity of the radius, comminuted by lines of vertical fission.

But the claim may be made that the comminution of the lower fragment is the result of impaction and penetration therein of the upper fragment, an explanation that has heretofore been generally accepted. But of the comminuted specimens before us not one presents any indication of impaction of the upper in the lower fragment. In one the separation of the two fragments from each other is still incomplete, the fragments remain still unseparated, with their periosteal investment intact posteriorly, and the anterior surface presents a gaping transverse fissure, up to which ascends from the articular surface a vertical fissure. The appearances presented by the specimen from Dr. Hamilton's cabinet are especially notable. From a central point in the concave articular surface radiate four fissures, which ascend vertically for two or more inches. No fragments have been split off, the bone would have been simply fissured longitudinally, were it not that there has been a transverse fracture, which has traversed the vertical fissure, and produced a lower fragment which is composed of several pieces. The precedence of the vertical fissures to the transverse fracture is unmistakable, and the very shapes and relations of the pieces of the lower fragment negative absolutely their production by the driving of the upper into the lower fragment.

In yet another specimen, there is a distinct indentation in the radial articular surface, with a stellate crack radiating therefrom, one of the rays of which reaches the posterior margin of the bone, and is discernible as a distinct fissure for an inch vertically.

The facts with regard to longitudinal fracture, which have now been demonstrated, may be thus summarized :

1. Direct impact of the carpus against the articular surface of the lower extremity of the radius tends to produce longitudinal fracture or fissure in the radius.
2. Longitudinal fracture or fissure only occurs where great original violence is inflicted.
3. Longitudinal fracture or fissure may occur alone, or may be complicated with transverse fracture or fissure.
4. Longitudinal fracture or fissure, when complicated with transverse, is produced first in point of time.
5. The force which produces longitudinal fracture or fissure in one case, will always tend to produce the same result, and will not sometimes produce the one and sometimes the other,

One hundred years have now elapsed since Pouteau first taught that injuries at the wrist from falls upon the palm of the hand, instead of being simply sprains, or incomplete dislocations, were fractures of the radius near its lower extremity; but I believe that I have the fortune of having now first recognized the full relation to these injuries of longitudinal fractures of the inferior extremity of the radius, and of elaborating their pathology. The pathology of Nelaton has completely blinded surgeons to the existence or true character of this injury. Bigelow, in 1858, had the clue which might have led to the establishment of the truth, but he did not pursue it. Two years later Lecompte made the most important advance in our knowledge of these injuries since the labors of Dupuytren, when in the *Archives Generales de Médecine* (1860, p. 653), he published his masterly and conclusive demonstration, that transverse fractures of the lower extremity of the radius were always due to *arrachement*, a force of avulsion, cross-breaking strain exerted through the anterior ligament of the wrist. Having this foundation which Lecompte has laid, it has been possible to add to it further true exact knowledge.

The discussion of longitudinal fractures of the inferior extremity of the radius had its origin in the hypothesis that in a given case the contraction of the flexor muscles was sufficient to overcome the tendency to extension which is impressed upon the hand and carpus as the immediate effect of a fall upon the palm of the hand. In cases where the excessive violence described as necessary to produce longitudinal fission fails to be inflicted, no evil effect beyond a certain amount of concussion is sustained by the structures of the wrist. But observation shows that in many cases, either the resistance of the flexors is overcome, or taken unawares they fail to contract at all, and the movement of extension going on, its force is brought to bear chiefly on the anterior radio-carpal ligament. The posterior surface of the first row of the carpus is now strongly pressed up into the posterior portion of the concavity of the articular surface of the radius, all pressure is removed from the anterior portion and is now wholly sustained by the posterior portion. The extended hand and carpus constitute a powerful lever, which, supported by the posterior wall of the radius as a fulcrum, exerts a resistless strain on the anterior ligament, and through it upon the anterior edge of the radial articular surface.

The result is either avulsion of that portion of the bone controlled by the ligament or rending of the ligament. The cross-breaking strain by which transverse fracture may be accomplished is thus produced. The full discussion of the mechanism of this force of avulsion, together with its demonstration upon the cadaver, and the development of certain new and important points in the pathology and treatment of the resulting in-

jury were contained in my memoir of March, 1878. I shall not repeat it. There is but little which I then said that later experience prompts me to recall or qualify. I stated then that almost invariably in experimenting upon the cadaver by producing over-extension of the wrist, the lower end of the bone was torn off. After a larger number of experiments I would make that less strong, and say that as a rule the ligament remains intact and the radius is fractured transversely at a point rarely higher than a half inch above the articular surface. In some instances, however, the ligament gives way and the radius remains intact, and occasionally one of the carpal bones is fractured. The shape and extent of the lower fragment torn off in these experiments has varied considerably. I have been able to produce almost perfect fac-similes of the various forms of the lower fragment already described as presented by the specimens of recent ante-mortem fracture.

By these experiments upon the cadaver, I have been able to determine the reason for these variations.

Of the three fasciculi which compose the anterior radio-carpal ligament, the middle is the weakest, and, if rupture of the ligament takes place, it is here that it begins. The outer part of the external band, where it is reinforced by the external lateral ligament, is the strongest. Thus it may happen that these fibres alone may remain intact, and the tip only of the styloid process be torn off; or, the entire external band remain intact, and the styloid process, with more or less of the articular surface, be torn off. If both bands remain untorn, the most frequent condition, the whole of the articular extremity will be torn off and a nearly transverse line of fracture be produced.

I have thus passed in review all the facts known to me, which bear upon the explanation of the occurrence of fractures of the lower extremity of the radius, with a candid desire to establish scientific truth.

The facts of anatomy, the facts of mechanics, the facts of pathology, of clinical experience and of experiment, combine to form the facts of the case upon which judgment is to be formed. All alike negative the theory that the ordinary transverse fracture of the lower extremity of the radius is the result simply of the bone having been caught between two counter-forces, the point at issue between my commentator and myself. These facts, however, have given us in response to our questionings a fuller and more accurate knowledge of the real pathology of the varying conditions which different cases of this fracture have been found to present. The outcome of the whole must be an improved therapeutics, more rational adaptation of treatment to conditions, more correct practice, better results, and a diminution of the danger, suggested by my friend at the outset of his article, that any reputable surgeon should be

unjustly ruined in reputation and prospect as the result of his treatment of a case of fracture of the lower extremity of the radius.

DISCUSSION.

DR. JARVIS S. WIGHT said that he would not detain the Society long, on account of the lateness of the hour. He had been very much interested, and he would also say instructed, by the elaborate and learned paper to which he had listened, and he took it as a matter of the highest compliment to himself or to any man to say that under any circumstances they would stand upon the truth, scientific truth, no matter how many strong men might be against them. That was his position, and it was his position *always*. The reader of the paper, therefore, paid him the highest compliment in this respect.

In regard to two or three points in the paper, about which, in the main, he did not feel that it was incumbent upon him to say anything, it would seem that he was called upon to make a few remarks.

He held in his hand a copy of the *Medical and Surgical Reporter*, of May 31st, 1879, in which he had reported a case that came to his clinique in the hospital. A bank of earth fell upon a man, who raised his right hand for the purpose of defending himself. The earth fell upon the back of the hand, striking the base of the radius, and thus making one of the most complete Colles' fractures he had ever seen. The length of the distal fragment was one-half inch. Another case was that of a little girl, six years of age, who, as she was coming home from Sunday-school, fell down stairs, and fell also on the back of the base of the radius, for there was a contusion, showing this to be the fact; but, instead of a complete Colles' fracture, it was an incomplete Colles' fracture. It was a splendid specimen of a "hickory stick" fracture; it held together and was bent over. This latter case might not be quite in order, but it was in that direction. This case is reported in the *Hospital Gazette*, April 5th, 1879. Here, then, were two cases in refutation of the statements made in Dr. Pilcher's paper. So much for that!

Again: he had heard something about mechanics. He wished to direct the attention of the Society for a moment in that direction, for he, too, had paid some attention to the subject of mechanics. Observe here—(illustrating) is a lever; his right hand is the power to the lever; his left hand is the lever; the muscles and anterior ligaments are the resistance which we call "weight." The base of the radius is the fulcrum. Therefore, by a simple rule of mechanics, the weight + the power = the resistance, or fulcrum. By the substitution of figures or algebraic expressions, a numerical value may be arrived at, say, $100 + 600 = 700$. This problem is one which any mere schoolboy can solve.

Again: there was one other point to which he desired to allude, but, before alluding to that, he would say that, in regard to the experiments detailed or spoken of in his paper, he had always spoken of them in the most complimentary terms which he was able to command. He had said that they were "admirable experiments," and he could not say more of anything. But he did say they were made forty-four years after the experiments of M. Bouchet, and that others had experimented in the same direction since that time. He repeated that he alluded to these in the most complimentary manner possible; but what he did object to was that these experiments were made upon dead bodies, and that they were incomplete and did not contain or convey the entire truth; that there were other causes which operated in the production of the results not detailed. He spoke of this in the paper referred to and gave it due weight in relation to

transverse fractures at the base of the radius. More than that, he claims that there are other cases of fractures, longitudinal fractures and impacted fractures, as for instance, transverse fractures, comminuted fractures at the base of the radius, and brought examples to prove that such was the case. And, as he looked at the diagrams which hung before him, he saw just one-half which proved his position in the most potential manner, especially several comminuted ones, and the other half did not militate against it. The ligaments may have been torn off; he did not say that they were not, but he did know that at the base of the radius we have a plain surface which will decompose a powerful force and give a resultant enough to break any kind of bone which can be imagined. He did not say that they were torn off; they may have been, but he could not see how they could have been comminuted any other way than by direct violence, by being crushed between two counter-forces. He could infer from observation that they might have been so.

In conclusion he reiterated his gratification because of the complimentary statements that were made, and because the diagrams seen before him were in a measure confirmatory of his own views. He would also say that between himself and his colleague there were no personalities; they were working for the same end—to ascertain the truth, and not only is there no feeling between himself and his learned colleague, but none between himself and his objectors in New York. In his estimation and hope they would all soon come to a better understanding of the whole subject. He did not profess to know all about it; but what he did profess to do was to get at something that would lift the surgeon into a better understanding of Colles' fractures, or fractures of the base of the radius.

DR. PILCHER replied that he felt personally greatly indebted to his friend, Dr. Wight, for having been the agent which had stirred him up to greater efforts to go a little further in his investigations in this matter. As a result there had been elaborated one of the most important points which has been made in this direction for a long time, and that is the elaboration of the pathology of longitudinal fractures at the base of the radius. This great consummation is largely due to the quiet and gentle position taken by his friend the Professor.

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AURAL SYRINGES.

BY J. S. PROUT, M.D.

As announced by the card, I shall confine my remarks to ear syringes, saying very little on the general subject of ear syringing, and shall address them to those who, as general practitioners, have not time or opportunity to make themselves expert in matters of aural practice. I hope that what I say may also interest our friends, the druggists, one of whom recently told me that "ear syringes" are sometimes bought by men who propose to use them on an entirely different part of the body!

As the subject is an important one, our patients are entitled to explicit instructions from us in respect of indications, instruments, etc. I therefore lay it down *as a rule, from which there is to be no departure*, that no ear should be actively syringed unless there is something in it that it is important to remove. It therefore follows, as a matter of course, that if the physician cannot examine the ear and determine that there is something in it that needs to be removed, he is not competent to advise for the case; and also that if, in despite of this inability, he prescribes syringing, he does so blindly, at hap-hazard, ignorantly, and is guilty of malpractice (bad practice), even though no appreciable injury is sustained by the patient.

What then are the things that we are to remove from the ear? They are *foreign bodies*, however they may have gotten in, *wax* and *pus*, which last may be either dried and caked in the auditory canal, or in process of constant formation and discharge.

Nearly all the objects that enter the ear from without may be removed by means of the syringe, and no one who has not given careful attention to the treatment of aural diseases should ever resort to any other method of removal.

When one reads and hears of the horrid work that has been and is still occasionally done by *legally* qualified practitioners in the search after foreign bodies that perhaps have no existence—drum-membranes destroyed, ossicles pulled out, etc., etc.—one is tempted to wish that some such unscrupulous incompetent may be sued for malpractice and mulcted in heavy damages—*pour encourager les autres!*

For the purpose of classification I shall divide syringes into 1, piston-syringes; 2, bulb-syringes in which the injecting nozzle is directly attached to the bulb; and 3, bulb-syringes in which the nozzle is connected with the bulb by a flexible rubber tube of several inches in length, the water being drawn into the bulb through a similar flexible tube, as in

Davidson's, or through a short metallic tube, as in the Derby or the American.

In the drug stores small cheap glass or pewter piston-syringes (made originally for the penis, perhaps) are sold for the ear, for which they are dangerous and almost worthless. I was told by a druggist the other day that a small glass syringe, of this sort, broke recently in a lady's ear while in use. The result was a cut on the cheek—strangely enough and very fortunately, the ear itself was not injured. Small hard-rubber piston-syringes of various styles are also sold for the ear, one of which, called the "ear syringe," is, in careful hands, a good instrument. I have used one of this sort, holding about $\frac{1}{2}$ ounce, for years, and find it sufficient for all ordinary purposes. When the piston does not work freely there is apt to be a jerky movement, and the delicate skin lining the auditory canal may be injured. It is also true that *no one* can use such an instrument satisfactorily and safely on himself. I therefore make the broad general statement that *for home and self-use, on the ear, a piston-syringe should not be used.* It is both dangerous and unhandy.

The different bulb-syringes of the first variety (second class) are better than those of which I have just spoken (first class), but they are less safe and less easily used than those of class 3, of which Davidson's may be ranked as first and best, the type of the good syringe for the ear *for home or self-use.* The power is applied by the hand to the bulb, and the force of the stream can be regulated with great nicety. The intervention of the flexible tube between the bulb and the nozzle prevents any possibility of damage through the force exerted by the hand, as may easily happen in using the piston-syringes. The nozzle may be held by the syringer or by the patient, with its point in the beginning of the external meatus. Any sudden movement of the head will be away from danger. This syringe, or one of this class, is to be found in almost every well-regulated household.

Another advantage of this class (3) is that when the syringe is in action, if the source of water-supply be raised it will act as a douche and give a steady stream of water to the ear, the force varying as the difference of level between nozzle and water-surface is great or small. Thus the Davidson syringe, besides its ordinary uses, is the best attainable domestic ear-syringe, and is as good a douche for the ear as we need, while the others of the third class are nearly as good.

The last addition to the family is Hall's Patent Health Syringe, which may be put into our third class. A glass jar for holding the water has attached to its top a rubber bulb, by means of which the air is condensed in the space above the water, which, through a tube that goes to the bottom of the jar and terminates externally in a flexible tube, is thus forced out in a steady stream at a moderate pressure.

What fluid is to be used in the syringe? Warm water of *about* the temperature of the blood ($98\frac{1}{2}^{\circ}$ Fahrenheit) is sufficient for all ordinary purposes. (*Nothing cold should ever be put into the ear.*) When there is a purulent discharge, a solution of borax (one or two teaspoonfuls to the pint) may be used, as it is both cleansing and healing. This or a solution of the carbonate or bicarbonate of soda is sometimes used to soften wax, as the alkali somewhat increases the solvent power of the water; but this is seldom needed. Castile soap may be added to the water; but, as a rule, it is better to avoid fluids containing organic material. (For the same reason it is unwise in earache to pour in oils, molasses or such substances, as they make the ear dirty, and so favor the development of disease. Warm water or this, with three times its bulk of laudanum, will usually do *more good and no harm.*)

It is *essential* that only *clean water* be taken into the syringe. This is easily managed by using two vessels, one a cup or finger-bowl to catch the water that flows from the ear, while in the other the clean water is held. By this precaution the syringe is always clean, and *cannot* carry infection from one patient to another.

Syringing the ear may sometimes produce disagreeable effects, such as cough, giddiness, or even fainting. Among other causes these may result from the use of water of too high temperature. Dr. Cornwell mentions a case in which faintness came on whenever the water used was "a little more than milk-warm." (*Cincinnati Lancet and Clinic*, Nov. 8, 1879, p. 360).

For ordinary syringing the temperature may be about 98° Fahr.; for the douche, for the relief of acute inflammation, it may be somewhat warmer, but in any case the feelings of the patient are the only safe guide.

Roosa (*Diseases of the Ear*, 4th Edition, 1878, p. 127) says: "I think the small hard-rubber syringe is the best, though a Davidson's syringe does very well." Turnbull (*Dis. of the Ear*, Phila., 1872, p. 86) advises Davidson's syringe, and says if this is not at hand a piston syringe (described and figured) may be substituted. I am sorry to say that he violates what I hold to be a cardinal rule in ear-syringing when he says (p. 89): "Wilde's basin is of great service in these cases. Its concave part fits accurately the curve beneath the lobe of the ear, and the perforated septum *strains the clean water from the dirty.*" (Italics mine.) This is disagreeably suggestive of dirty instruments, and of the communication of disease from one patient to another. Von Troeltsch says: "There can be but one object in syringing the ear; that is, the removal of something from it. You will be still more surprised when in your practice you find that almost every aural patient, who has not come to you at first, has been ordered to syringe the ears. The patients who tell you this will

often very earnestly and truly tell you that nothing was removed." (Trans. by Roosa, 2d Am. Edition, N. Y., 1869, p. 93.) Burnett (Diseases of the Ear, Phila., 1877) only mentions the piston syringe, and repeats the same advice in his "Hearing; and How to Keep It" (American Health Primer, No. 1, Phila., 1879) an excellent little book, of interest to us all, and one that I wish I could induce every member of this Society to read. It would then be well for them and better for their patients. I also wish that it could receive the wide general reading that it deserves, and for which it was intended. Those with ear trouble would then better appreciate the quality of the advice they receive. Light is good in dark places. *The influence for good on the profession, of correct ideas among the laity, cannot be over-estimated.*

In conclusion, let me say that I always protect the reputation of a professional brother if possible. Let me cite a case to show why this cannot always be done: A very intelligent patient came to me for treatment, saying that his physician had assured him that there was *no* wax in his ears. I found each ear nearly full of wax, which I was obliged to syringe out. I did not comment on this unfortunate contradiction, nor did he, but it is not safe to infer that he did not notice it, and did not draw his own conclusions.

The instinct of self-preservation prompts patients to respect a physician who candidly says he does not understand some disease of theirs that is entirely out of his line, and he maintains his own self-respect by doing so. But how does the case stand when the patient discovers that the physician professes to understand matters concerning which he is perhaps entirely uninformed? Should not each man know and honestly recognize his own limitations?

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A REGULAR STATED MEETING was held on the evening of Tuesday, May 18th, 1880, at No. 398 Fulton St., Brooklyn. The President, CHARLES JEWETT, M.D., in the Chair. DR. WYCKOFF, Secretary.

The MINUTES of the previous regular meeting and of the special meeting were read, and after correction of the minutes of the regular meeting, held April 20th, approved.

The following physicians were duly nominated to membership:

Augustus F. W. Reimer, M.D., 295 So. 5th St., E. D.; Chas. H. Wilson, M.D., 86 Clymer Street, E. D., both by Dr. Russell; G. A. Cardwell, M.D., 85 Bedford Ave., by Dr. Baker; Alf. S. Houghton, 1262 Fulton, by Dr. H. F. Williams; Geo. McNaughton, 20 Greene Avenue, by Dr. Rochester.

The COUNCIL reported the acceptance to membership of the following duly nominated candidates: R. T. Davidson, M.D., Bell. H. M. C., 1877; Thos. Gallaher, M.D., Bell. H. M. C., 1879; P. J. Prendergast, Jeff M. C., 1873.

The PRESIDENT stated that in pursuance of Chapter 12, Section 3, of the Revised By-Laws, he would formally declare Drs. W. E. Conway, C. L. McCann and C. H. Johnson members of this Society.

The PRESIDENT also stated that by special request the programme of the evening had been transposed, and the "Report on Measles, with special reference to the recent epidemic, by the Committee on Hygiene," would be read first.

DR. JEROME WALKER, Chairman of the Committee, before reading the report, remarked that it was but justice to the other members of the Committee to state that the report was the combined work of all the members—although, of course, it was written and arranged by the Chairman; yet the report had been wrought out by the individual members of the Committee.

The report was then read and discussion was held by Drs. Raymond, Walker, Hart and Sherwell:

DR. PROUT, announced to read a paper on "Aural Syringes," made a few extempore remarks upon the subject. He would elaborate them from the stenographer's notes.

DR. L. S. PILCHER read a paper entitled "Recent Views on Fracture of the Inferior Extremity of the Radius." This paper was discussed by Dr. Wight.

THE CHAIR then called for the "Notice of the Meeting of the Convention at Washington, for the Revision of the U. S. Pharmacopeia, held May 14th, 1880, by Dr. E. R. Squibb.

DR. SQUIBB begged to be excused from longer detaining the Society, on account of the lateness of the hour, and because full accounts of this Convention will be found in the various medical journals.

No formal action was taken upon Dr. Squibb's suggestion.

THE CHAIR announced the presence of DR. F. STURGES, of New York, as an invited guest, and of DR. D. B. WHITNEY, bearing credentials as a delegate from the Medical Society of the County of Queens. He invited these gentlemen to seats by the side of the presiding officer, and extended to them the heartiest welcome on behalf of the Medical Society of the County of Kings. He hoped Dr. Whitney would convey to his Society the cordial greetings of their fellows in the profession.

DR. WHITNEY responded in his happiest vein, saying that he bore a commission from the Medical Society of the County of Queens to the Medical Society of the County of Kings, which was a sufficient guarantee of a reciprocal brotherly love and regard between these two representative medical organizations, and an earnest of continued cordial relations.

DR. A. E. M. PURDY, of New York, who was to be present at this meeting, did not appear.

THE CHAIR announced the death of Dr. John W. Hamilton, a member of the Society. An appropriate obituary committee would be appointed hereafter.

DR. JEROME WALKER moved the following:

Resolved, That a committee consisting of Drs. W. F. Sanford, Jarvis S. Wight and Geo. K. Smith, in conjunction with the President of the Society, be appointed, for the purpose of calling a meeting, special or otherwise, to consist of lawyers and physicians, for the discussion of such medico-legal points as the committee, in their wisdom, might see fit.

In explanation he said this motion was made partly because of the recent trial for malpractice, in which all had been more or less interested, and partly because many had expressed a desire for such a meeting. Papers read by a lawyer and a physician would undoubtedly bring out some valuable information.

The Society then adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρωποῖσι, κακῶν θελκτῆρ' ὀδυνῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—LIBRARY FUND SUBSCRIPTIONS have been received from Drs. Whiting, Gray, Arnold, Conkling, Atkinson, Shaw, Westbrook, Wright and Carreau.

—PURE FRUITS, ETC.—We commend to the notice of the physicians of South Brooklyn, as well as all other sections, a most praiseworthy undertaking on the part of a lady residing at 393 Degraw St., to supply all kinds of pure preserved fruits, vegetables, jellies, jams, pickles and pastry. "Absolutely pure" is the motto; no gelatine, coloring or other "enriching" abomination being employed. For descriptive circular and price list, address Mrs S. S. McElrath, at above number; or call and examine specimens.

—QUARTERLY EPITOME OF PRACTICAL MEDICINE AND SURGERY, being an American supplement to Braithwaite's Retrospect, containing a retrospective view of every discovery of practical improvement in the Medical Science, abstracted from the current medical journals of the United States and Canadas. Part 1, March, 1880. N. Y.: W. A. Townsend. \$2.50 a year in advance. Quarterly parts, 75 cents.

SEE PAGE 184.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The June meeting will be held on the 15th, at which there will be presented the following papers:

A Remarkable Case of Chloral Inebriety, by Dr. J. B. Mattison. History of a Case of Chronic Arthritis of the Knee Joint, Followed by Amputation.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President</i>	C. JEWETT, M.D., 307 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
<i>Secretary</i>	R. M. WYCKOFF, M.D., 532 Clinton Ave.
<i>Assistant-Secretary</i>	J. H. HUNT, M.D., 419 Hart St.
<i>Treasurer</i>	J. R. VANDERVEER, M.D., 301 Carlton Ave.
<i>Librarian</i>	A. HUTCHINS, M.D., 796 De Kalb Ave.

CENSORS.

B. A. Segur, M.D., 281 Henry St.	F. W. Rockwell, M.D., 6 Lafayette Ave.
E. R. Squibb, M.D., 36 Doughty St.	L. S. Pilcher, M.D., 4 Monroe St.
J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK.

(1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI., Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Dr. R. M. Buell
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

DEAR SIRs:—It is with great pleasure that I report to you briefly my experience, and also that of eminent Physicians, as to the valuable medicinal qualities of *Ingluvin*, and testify to its superiority in all cases, over Pepsin.

Drs. F. A. Howe and E. P. Hurp, of Newburyport, Mass., report a case of Chronic Dyspepsia, as follows: Our associate in medicine, Dr. E. Cross of this city, was taken violently sick. For a time his life was despaired of; everything was tried, but with no good effect. Finally, *Ingluvin* was administered in doses from 5 to 10 grains; to our surprise, the patient began at once to mend, and rapidly convalesced. We cannot speak too highly of *Ingluvin* in this case; it is certainly a valuable remedy.

Dr. F. W. Campbell, of Montreal Canada, says that with *Ingluvin* he cured three out of four cases of *Vomiting in Pregnancy*.

Dr. C. F. Clark, Brooklyn, N. Y. has used *Ingluvin* very extensively in his daily practice for more than a year, and has fully tested it in many cases of *Vomiting in Pregnancy*, *Dyspepsia*, and *Sick Stomach*, with the best of results.

Dr. Edward P. Abbe, New Bedford, Mass., mentions a case of Vomiting, caused by too free use of intoxicating liquors; *Ingluvin* was administered in the usual way, the effect was wonderful, the patient had immediate relief.

A gentleman living in Toronto, Canada, gives his experience. He says, I was suffering terribly from Indigestion, I could eat nothing, life was almost a burden to me. *Ingluvin* was prescribed in five to ten grain doses, the medicine was taken for about two weeks; result, a permanent cure.

In fact were I to note all the remarks of the profession and my experience in relation to this remedy, and report to you the cases in detail, you could fill a volume with expressions as to its great efficacy in the troubles for which it is recommended.

Yours Respectfully,

CHAS. H. BENNETT,

August, 1878.

144 Luqueer St., Brooklyn, N. Y.

PHYSICIANS SHOULD ORDER W. R. WARNER & CO.'S SOLUBLE SUGAR-COATED PILLS IN BOTTLES CONTAINING 100, AND OBSERVE THE TRADE MARK ON EACH LABEL AS A GUARANTEE OF THEIR RELIABILITY.

A PERFECT SUGAR-COATED PILL IS THE MOST
CONVENIENT AND DESIRABLE
FORM OF MEDICINE.



ODD NUMBERS OF JOURNALS NEEDED TO COMPLETE FILES.

THE LIBRARIAN would be very glad to receive copies of the following journals, or would be pleased to negotiate for the same. A great favor to the Library would be conferred:

AMERICAN JOURNAL MEDICAL SCIENCES.

1858.—All.

1865.—January, April.

NEW YORK MEDICAL JOURNAL.

1856.—July.

1858.—May.

1872.—June to December, inclusive.

PSYCHOLOGICAL JOURNAL.

Volumes V. and VI.

PRACTITIONER (LONDON).

Volumes I. to V., inclusive.

TRANSACTIONS AMERICAN MEDICAL ASSOCIATION.

Volumes 4, 14, 17, 18.

LANCET.

1850.—January to June, inclusive.

1855.—June, July, August.

AMERICAN JOURNAL OF OBSTETRICS.

Vol. 1.—No. 1.

Vol. 7.—Nos. 3, 4.

Vol. 2.—No. 2.

Vol. 8.—Nos. 1, 2, 3, 4.

Vol. 3.—No. 3.

Vol. 9.—Nos. 1, 2, 3, 4.

Vol. 5.—Nos. 2, 3, 4.

Vol. 10.—Nos. 1, 2, 3.

Vol. 6.—Nos. 1, 2.

Vol. 12.—Nos. 2, 3, 4.

RANKING'S ABSTRACT OF THE MEDICAL SCIENCES.

Vol. 2, No. 1.—January to June, 1846.

Vol. 39.—January to June, 1864.

No. 16.—July to December, 1852.

Vol. 40.—July to December, 1864.

Vol. 36.—July to December, 1862.

Vol. 41.—January to June, 1865.

Vol. 37.—January to June, 1863.

Vol. 42.—July to December, 1865.

Vol. 38.—July to December, 1863.

Vol. 43.—January to June, 1866.

MEDICAL AND SURGICAL REPORTER.

All back of 1865, inclusive.

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

JULY, 1880.

No. 5.

HISTORY OF A CASE OF CHRONIC ARTHRITIS OF
THE KNEE JOINT, FOLLOWED BY AMPUTATION
AT THE LOWER THIRD OF THE THIGH.

BY G. W. BRUSH, M.D.

E——S——, aged nineteen, a native of the United States. Family history good.

This patient came under my care on the fifteenth of January, 1880. He had been treated previously by various doctors, changing from one to another, as such cases are apt to do. The history of the case was about as follows:

Some time in 1877, while playing with some boys, one of them came up behind him and tried to throw him by suddenly pushing his knee into the popliteal space. In the attempt to save himself from falling the joint was badly strained. No surgical advice was had, and, though suffering a good deal of inconvenience, he managed to get about with the aid of a cane. About a year afterward he fell, in going down stairs, and thus the joint received a new injury, so that he was obliged to resort to the use of a crutch and cane. Various domestic remedies were applied, and a year previous to my seeing him electricity had been applied by an irregular doctor. His condition was such as might be expected. He was emaciated, but suffering no pain of any account. Appetite good. The joint was very much swollen, the tissues tense, but not specially painful to the touch. There were occasional spasms of the muscles, especially while sleeping. No crepitation could be discovered, nor

any signs of fluid. The limb was considerably flexed, and the head of the tibia drawn backward. Amputation had been advised before I saw him. His condition was not such as to lead me to advise immediate amputation, and I resolved to do what I could to save the limb, in the meantime building up the patient with tonics, and thus prepare him for the amputation, if unsuccessful in the attempt to save the limb. Syrup of the iodide of iron and quassia were given in moderate doses, and a dessert spoonful of cod liver oil three times a day, and absolute rest enjoined. The joint was also painted with tincture of iodine once a day. The compound iodine ointment was applied at night for a time, but the patient complained so much of the intolerable itching produced by it, that its use was abandoned.

The patient improved rapidly in flesh, gaining over a pound a week. (He *would* go down stairs once a week to weigh himself.) The joint measured, on the 15th of January, 1880, when first seen, fifteen and seven-eighths inches in circumference. On the 1st of Feb. I applied a bandage quite tightly, and the limb seemed to be more comfortable, there being less spasm of the muscles.

Feb. 2d, the bandage was removed and joint measured fifteen and one-quarter inches.

Feb 3d, the plaster bandage was applied, carrying it about four inches above and below the knee joint. From this time I saw the patient every other day. Tapping had been advised by others, but from the fact that I could detect no fluid, I decided not to do so. My friend, Dr. E. Reynolds, of this city, saw the case with me and concurred in this opinion. Feb. 8th, the patient had gained over five pounds in weight. There was no marked improvement in the condition of the joint however; on the contrary, the swelling seemed to be extending upon the thigh, and I now felt that the time had come to make the final decision with reference to amputating, and called in my friend, Dr. Jarvis S. Wight, of the Long Island College Hospital, whose opinion I had long ago learned to respect, and his advice was to amputate.

I informed the family of the decision we had reached, and taking the circumstances into consideration, it was thought advisable to remove the patient to the hospital. Prof. Wight kindly offered to admit him to the L. I. C. H., and he was admitted on the 24th of Feb., and the next day the amputation was made by Prof. Wight in the presence of the medical class, the patient being anæsthetized with ether. There was very little shock from the operation and subsequently no surgical fever. The second day after the operation the knee joint was prepared for examination by Dr. Benj. F. Westbrook, and its condition revealed the facts that the amputation could not have been made at a more opportune time.

There was tubercular infiltration of the soft parts and caries sica of the head of the tibia and head of the femur. There was no fluid present, showing the wisdom of the decision not to aspirate.

The patient rapidly recovered, having no drawbacks whatever.

One of the things which I desire to call especial attention to in this case is the marked success of the dry method of dressing. The flaps were brought together with three wire sutures and a drainage tube inserted. The stump was then covered with marine lint saturated with carbolized oil, and two sand bags of about one pound weight each were laid upon the parts to control hemorrhage of the small vessels and spasm of the muscles. No water was used at any time, the waste products of repair being carefully wiped away with marine lint saturated with the carbolized oil; in fact, the stump was disturbed as little as possible, this being one of the features of the treatment.

The patient was supported with tonics and the usual diet in such cases.

Much of the credit for the marked success of this case is due to the care and kindly attention bestowed by Dr. F. E. Wilson, the house surgeon, who faithfully carried out the details in every particular.

—LETHAL FOG.—In London the rate of mortality for the week ending February 7 was registered at 48.1 per 1,000 inhabitants. This enormous ratio represented a total mortality of 3,376, or over 50 per cent. higher than that of the preceding week, and nearly 100 per cent. higher than the weekly average in 1879. It was without precedent since the cholera season of about 30 years ago. The diseases especially fatal were those affecting the organs of respiration. These caused not less than 1,557 deaths, 1,223 of which were due to bronchitis. This excessive mortality was, in great measure, referable to climatic conditions, and is spoken of as the "fog mortality" of London. The weather was cold, and fogs had been dense and continuous. Cold alone does not account for the disaster, since in several of the English towns the cold was equally, and even more severe than in the metropolis, and yet their death-rate was not unduly increased. The mischief appears to have been mainly wrought by fogs, heavily charged with the "London smoke," or carbon in a minutely divided form, the properties of which are intensely irritant to the respiratory passages. The deaths of aged persons were unusually numerous in the week mentioned, largely caused by pulmonary affections. Conjunctival irritation was very prevalent, due to the action of the finely divided carbonaceous particles.

A REMARKABLE CASE OF CHLORAL AND CHLOROFORM INEBRIETY.

BY J. B. MATTISON, M.D.

Through the courtesy of Dr. J. M. Raub, of this city, I have the pleasure of presenting the history of a case of chloral and chloroform inebriety—notable not from the excessive quantity or duration of the chloral or chloroform taking, but in view of the profound and prolonged functional brain disorder they occasioned, in which respect it is unusual, and deserving of more than a passing notice.

Miss B. D., age 22, at the age of 11 began to experience ill-defined attacks of nervousness, which were attributed by her physician to approaching menstruation, which commenced in her 12th year, and continued regular and easy till the age of 15, when, after indulging in horse-back exercise, she began to suffer from dysmenorrhœa, which persisted in varying intensity, accompanied with marked nervous irritability and insomnia until the middle of March, 1879, when she was ordered by her physician 3 grains of chloral every 4 hours for relief of her sleepless and nervous condition. At the following period the dose was increased to 9 grains every 10 minutes till three doses were taken, and then every half hour until relieved. The nervousness persisted through the following interval with increasing prostration, but the chloral was continued at intervals of 10 minutes to 2 hours, until, at the end of 6 weeks, she was taking 192 grains daily.

During the 6th week hallucinations of sight set in, but gave rise to no alarm, being thought due to a small quantity of opium in a prescription given by her medical adviser—not the one who prescribed the chloral—who supposed her suffering from malarial fever. The hallucinations increased till Sunday, May 18th. Her physician had, meanwhile, received a copy of my paper on Chloral Inebriety—read before this Society in April of last year—and brought it to the attention of the patient's mother, a very intelligent lady, who, divining the cause of the mental disturbance, insisted on discontinuing the chloral, and it was at once withdrawn. The same night a typical attack of delirium set in, with maniacal delusions and great physical distress, until 3 o'clock the next morning. She was given 3 15-grain doses of chloral every 15 minutes, which secured a 3 hours' sleep. On awaking, the pain and delirium returned with increased violence, and no more chloral was given. This condition continued for 10 days, with scarcely any intermission. The insomnia was intense—not more than 5 minutes' sleep at a time—and while, prior to

the chloral withdrawal, her prostration was so marked that she was unable to leave her bed without assistance, she now became so violently restless that she could scarcely be kept quiet, and her talkative delirium was excessive.

During the first week her bowels were torpid; breath offensive; teeth and lips covered with a yellow viscid coating that returned as often as removed; eyes painful and congested, and skin pale. During the first 6 days of abstinence, was given cathartics freely, and the following day—one week from date of chloral withdrawal—placed on tinct. iron and strychnine. Four days after, began to improve; the first indications being a lessened insomnia and decreasing violence of the mental and physical distress. On the 13th day she again became violent, seemingly caused by the noise of a carpenter employed about the house, who was compelled to suspend work and assist in restraining her, to prevent her going on the roof in search of her mother, who was absent. Improvement, with occasional relapses, continued during the 3d week, until the 21st day, about 4 P. M., when the delusions and mania returned with severity, persisting till 9 o'clock, when she retired and slept well.

Twenty-third and twenty-fourth day, mental disturbance quiet but constant; various delusions; imagined she was her mother. During the fourth week improvement continued until the night of the twenty-eighth day. She asked for a lunch and ate with appetite, though not largely, which was the first solid food—eggs beaten with milk being her entire diet, and that almost under compulsion, during her four weeks of positive delirium. Thirtieth day, improving, but still has hallucinations of sight and sound, though now, for the first, quite conscious that they are such. Four weeks later. Has been and is continually haunted by sights and sounds which she knows are not real, but, nevertheless, distress her extremely, the voices, especially. Her sleep, though with troubled dreams, is good, better than since she was seven years old, when, it may be noted, she had an attack of acute rheumatism from which she recovered with a damaged heart, that, during her chloral taking, instead of being worse, seemed greatly better.

We come now to an interim of several months in which no chloral was taken. Her physical condition greatly improved, yet she was still somewhat nervous during the day, mainly at her periods; and her sleep, though dreamy, continued good.

About Nov. 1, she began the use of chloroform by inhalation, having discovered its soothing effects during one of her painful attacks by smelling a small quantity of toothache drops containing it. Her mother then consulted a druggist as to the danger of using it, and his reply being satisfactory, he prepared a combination of chloroform and alcohol, two

ounces to a pint, which was used at first so freely as to cause vomiting, and after that, on retiring and during the night to procure sleep, which, up to the time of her commencing it was good, but, subsequently, sleeplessness became persistent and progressive.

Thus, through November, December and January, until the 20th, when, during a very painful period, she inhaled, in the course of eight hours, the entire pint of alcohol and chloroform, causing frequent emesis, but immediately resuming it, the pain seeming to overcome her horror of the nausea and vomiting. This was the climax of the chloroform. No more was taken, and the following day hallucinations of sight and delusions set in, under the influence of which she jumped from a window twenty feet to the ground, and was picked up unconscious, remaining so for forty-eight hours. During next three days was very delirious and greatly prostrated. Sunday and Monday delirium less decided until 5 P. M. of the latter, when it increased to great violence. Tuesday, January 27, seemed in much distress of mind and body; eyes congested; breath offensive; skin waxy; teeth and lips sticky; bowels torpid, and obstinate rejection of all solid food. This state of affairs persisted till Friday, February 6, 1880, at 8 P. M., when Dr. Raub was called to see her. He found her very delirious, constantly begging for stimulants or something that would end her life; saying she was being tortured by horrible sights; conjunctivæ yellow and congested; pupils dilated and feebly responsive; face puffy and extremely pale; tongue coated; breath offensive; hands and feet very cold and bathed in clammy perspiration; respiration irregular and spasmodic; pulse very feeble—86 to 92—and temperature $96\frac{1}{2}$.

She was ordered 30 grains of bromide potass. with \mathfrak{Z} i. elix. valer. amonia, to be repeated in an hour and a half. Visited at 11 o'clock and was quietly sleeping, but so extremely prostrated that it seemed doubtful if she would survive till morning. Medicine continued every three hours, unless sleeping.

Saturday, February 7, 8 A. M.: P. 84; R. 24, irregular; T. 97. Still very delirious and begging for stimulants. Bromide continued, and two C. C. pills ordered. In the evening learned there had been two clay-colored evacuations; had vomited freely and passed a large amount of urine, clear and limpid like hysteria, without albumen, and s. g. 1006.

February 8: Had a better night; less delirious and able to connect her words; eyes less congested; pupils dilated; face more natural; tongue edges cleaning; breath not so odorous. P. 80 and weak; R. 20 and regular; T. $97\frac{1}{2}$. Bromide continued, with milk and beef tea ad. lib. Evening visit, improving.

February 9: Passed the night fairly well. Is gradually gaining; hands and feet still cold, remaining so for several days. Takes an inordinate quantity of liquid food—12 quarts in 24 hours. Complains of wandering pains—"feeling as if there was a band circling the limbs in different parts."

February 10: Rested well. P. improving; R. natural; T. 98. Constant desire to urinate, and much difficulty in doing so. Bromide every 6 hours, and the following pill thrice daily:

R. Quin. Sulph., Zinc. Valerian, aa. grs. iss.; Ferri. Lact., grs. ii.; Strych. Sulph., gr. 1-40; Zinc. Phosphid., gr. 1-12.

February 11: Still improving in every respect, though marked symptoms of hysteria have developed; is inclined to cry incessantly, and passes large quantity of urine.

Prescribed with pill and bromide: Tinct. Assafoetidae, Tinct. Castorei, aa. ℥i.; Tinct. Val. Comp., ℥ss.; Mist. Caurph., ℥i., every four hours, with excellent result.

February 12: Rested better than any previous night, and seems quite like herself. Talks much better, though unable to converse to any extent or carry out a consecutive line of thought. Memory almost lost; could not tell her age.

February 13: Improvement continues. T. 98½; P. 76; R. normal; hands and feet nearly natural. Ordered up a short time, but is so weak that she is helped from bed to chair. Complains of girdling pains, and spinal pressure eliciting several points of irritation. Applied a blister, which seemed to relieve. Bromide and Assaf. Mixture continued.

February 20: Improving; eyes natural, except slightly dilated pupils; tongue clean; P., R. and T. normal, and for the first time able to converse satisfactorily regarding her illness, which was a perfect blank, and to tell her present condition. Said she had horrible dreams—her life threatened, and she powerless to resist. During the day varied delusions, with hallucinations of sight and sound. Expression listless and vacant, imbecilic in general appearance; aimless wanderings. Extreme loquacity and marked irritability.

February 28: Amendment continues, more marked in body than mind, though she now realizes her mental condition, and wills to do as desired. Complains of pain in head and eyes. Re-apply spinal blister with relief.

March 4: Somewhat restless at night. Order Brom., Lithium and Potass., 10 grs. each, one hour before retiring, repeated in 3 hours if required. Resumed Assaf. mixture thrice daily; also Percy's Vitalized Phosphates.

March 10: Improving, and looking forward to time of menstruation,

which had been suspended during 8 weeks, in hope that then all her mental cloud would vanish.

March 14: Severe dysmenorrheic attack, requiring morphia and Brom. Potass., the latter every 4 to 6 hours for two days; but only one dose, $\frac{1}{4}$ gr. of morphia, which created strong desire for its repetition. This attack subsiding, the bromides were given only at bed-time, repeated during night if needed, and the Vital. Phos., with tonic pill continued.

March 20: Bromism developed. Suspend bromides and substitute syr. lactucarium, \mathfrak{Z} ii. doses with excellent hypnotic effect.

March 25: Improving—body more than mind. Delusions less general, centering on three distinct lines of morbid thought: first, must support herself by becoming a servant, and seems determined to seek a position; second, conceived a great dislike to her mother, and censured her for the indolent life she had been allowed to lead; and thirdly, much concerned over a great political contest, with battles fought in her presence, and an impending religious warfare between Catholicism and Protestants.

April 1: The political and religious delusions persist; the others subsiding.

April 6: Is easily irritated, and the delusions and irritability seem closely connected, one seeming to provoke the other. There is a strong tendency to melancholia and suicidal propensity if delusions do not soon disappear. Rest good, except horrible dreams.

April 9: Visited theatre, enjoying the performance as before her illness, and seemed perfectly rational.

April 13: Became greatly excited and struggled violently with her mother for three hours, saying she had felt insanity coming for several years; that every physician had pronounced her insane, and she insisted on being immediately removed to an asylum. It was almost impossible to convince her that such was not her condition.

April 13: Menstruation began, which seemed to dismiss the whole thought of insanity.

June 15: During past two months has progressively improved. No hallucinations, but occasional mild delusions, which she is able to dismiss at will. Strength and appetite normal. Sleep good, though some annoying dreams. Has passed last three periods without pain, and expresses herself as "feeling better than for years."

Such, gentlemen, are the details of a case of no common interest, and one deserving a few special comments. It affords additional proof of the subtle power and baneful effects of chloral, and again refutes the statement that it will not create a so-called "habit." Dr. H. H. Kane, who

is collecting facts relative to chloral, has informed me that many cases of this kind have been reported to him. I personally know of such instances, and any physician prescribing the continued use of chloral does so at his patients' peril. Especially is this true of American women, so many of whom struggle through years under the burden of uterine or nervous derangement, and who find transient relief in the snaring solace of chloral, opium or alcohol.

The striking similarity in the symptoms of chloral and chloroform taking in this instance will repay careful study and afford additional presumptive evidence that the former acts by evolving the latter. And in this connection allow me to again call your attention to one feature not infrequently noted in these cases—the peculiar pains, simulating neuralgia or rheumatism, yet unlike them in not being limited to the nerve or joint, but *around* the limb, *girdling* it, as it were, and when complained of should always excite suspicion of chloral or chloroform taking. They are sometimes very unyielding, lingering in one limb for weeks, but little affected by treatment, and seemingly subsiding in direct relation to the toning up of the general system.

An interesting feature of this case is the present freedom from dysmenorrhœa, after nearly seven years' continuance. Does not the fact that no local treatment was made use of and that improvement followed the bromidal medication make it fair to infer that it contributed mainly to her cure? Certain it is that there may be varied departures from health referable to the uterine region without any uterine disease, but dependent on spinal hyperæmia and irritation, in which the continued use of the bromides, by removing the local congestion, afford permanent relief. On this score this case is suggestive.

The practical teaching of this and kindred cases is obvious. The profession must rise to a proper appreciation of the danger incident to an indiscriminate prescribing of narcotics, if the mothers and daughters of the future are to enjoy an immunity from one phase of suffering denied their sisters of to-day.

—ROUSSEAU is represented by Dumas as saying of medicine: "It is a noble profession. In it you may choose between real science, ever modest and self sacrificing, and quackery, ever noisy and empty. If you would become a physician, young man, study; if a quack, nothing but impudence and effrontery are necessary."

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A regular stated meeting of the Medical Society of the County of Kings was held on the evening of Tuesday, June 15th, 1880, the President, Dr. Charles Jewett, in the chair. Dr. Wyckoff, Secretary.

About 45 members were present. The meeting was called to order at about 8:26.

The minutes of the previous meeting were read and approved.

DR. H. HARDRICH proposed for membership A. G. Meyersburg, M.D., University of New York, 1875, of East New York.

THE COUNCIL reported acceptance to membership of the following :

Augustus F. W. Reimer, M.D., Göttingen, 1860; George McNaughton, M.D., Bellevue Hosp. Med. Col., 1878; Wm. Waterworth, M.D., Bellevue Hosp. Med. Col., 1878; Melville W. Barnhart, M.D., Univ. of N. Y., 1879; Alfred S. Houghton, M.D., Col. P. and S., N. Y., 1880; Charles Henry Wilson, M.D., Univ. of N. Y., 1876; John A. Arnold, M.D., Bellevue Hosp. Med. Col., 1871.

THE CHAIR stated that, by special request, Dr. Brush's paper would be read first.

DR. G. W. BRUSH then read a paper entitled, "History of a Case of Chronic Arthritis of the Knee-joint, followed by Amputation." In connection with the paper he exhibited a plaster of paris cast, which showed the condition of the limb at the time of the amputation.

DR. J. B. MATTISON read a paper entitled, "A Remarkable Case of Chloral and Chloroform Inebriety."

DR. RAUB, being called upon for remarks, had very little to say as to his connection with the case, in addition to what had already been said by the author of the paper. The interesting point to him was whether the bromide controlled the hyperæmia of the cords enough to relieve the uterine troubles.

The paper of DR. D. B. WHITNEY, of Queens County, entitled, "A Case of Aneurism of the Splenic Artery," in his absence, was read by title.

DR. J. H. H. BURGE, Chairman, presented the Report of the Committee of Dr. Prout's paper on Hospital Management. On motion, the report was received.

The Committee appointed to consider the subject of Dr. Prout's annual address, touching a point of hospital management, beg leave to report very briefly, rather with the views of eliciting a general discussion of the subject than of throwing any new light upon it. The evil, of which Dr. Prout complains, he has formulated in the following language :

"A private patient in a general hospital pays for his board, etc., but pays nothing for medical services, and the physician who attends him is paid nothing—that is, the patient is necessarily a pauper, so far as the medical man is concerned, as the latter can make no charge for services rendered. Consequently, a physician who has a hospital appointment can obtain for his patient the advantages of a private room in the hospital only by giving up all pecuniary interest in the patient, while a man without a hospital appointment, or with one in the wrong place, can do so only by the entire loss of *both* patient and fees. This should not be. It is bad for patient and medical attendant; it is worse for the hospital and the medical men on its staff. Hence," says the doctor, "without any qualification or reservation, I lay down the following

"PROPOSITION: A general hospital should have private rooms, the patients occupying which may choose their own medical attendants, whether on the hospital staff or not, paying them for their services at their usual rates of charging. The hospital shall furnish such patients with all other necessities at the ordinary published rates for private patients."

Now, we have no hesitation in saying that we are in sympathy with this general proposition, but, as it involves important questions, ethical, professional and practical, we are not quite so clear in our views as to the best method of inaugurating the reform suggested.

The establishing of first-class invalid boarding houses, where the best nursing may be had, is certainly a great desideratum, and this need may no doubt be largely supplied by private wards in the hospitals already existing; indeed, we are happy to say that such an arrangement already obtains in many hospitals, the managers of which have not cared to commit themselves publicly to the admission of practitioners not on their regular staff. In this connection we would remind the members of the Society that hospital management is seldom in the hands of medical men, and that the staff generally are subject to annual election, and have only a nominating voice in the appointment of new associates. It is within the knowledge of your Committee that very recently a meeting of the staff of a hospital was called to discuss the very subject which is now before us, and, after full discussion, they unanimously concluded to defer action thereon indefinitely.

Dr. Prout has supported his views by reference to the Washington City Infirmary, the Carney Hospital, in South Boston, and St. Luke's Hospital, in St. Louis, where the plan which he proposes has worked long and well. To this list may be added St. Vincent's Hospital and the Church Home, at Baltimore; Rochester City Hospital, in this State, and other institutions, by special arrangement.

We believe that much good will be effected by the general attention which Dr. Prout's address has called to this important subject; but the exact plan of operations must be left to individual institutions.

The following outline of a working plan, as given by Dr. Prout, we transcribe as a basis which may be modified to suit local necessities:

"1. The attending staff shall have full medical control over the patients in the wards.

"2. Patients who take private rooms shall be attended gratuitously by the attending staff on duty at the time; or they may choose their own physicians, whether on the staff or not, paying them for their services as if in a private house.

"3. Any member of the Medical Board, or any regular practitioner in good standing recommended by one of the Board, may attend private patients, as provided for in the second clause of section 2.

"4. The Medical Board may prescribe the hours during which private patients shall be visited by their physicians, and may, at any time, revoke this privilege as to any practitioner.

"5. In order to prevent all misunderstanding, each receipt given to a private patient shall specify that medical attendance is to be furnished by the hospital; *or*, that the patient, having chosen his or her own physician, is to pay such physician for his or her services."

J. H. HOBART BURGE, M.D.,
JARVIS S. WIGHT, M.D.,
J. C. HUTCHISON, M.D., } *Committee.*

The report being before the House for discussion,

DR. PROUT remarked that he did not desire to say much in addition to what he had already said in connection with the subject, or to offer any plan different from that which he had suggested. He had, in the beginning of his medical career, an opportunity to know that the plan was practicable; and, since his residence in this city, he had often wondered why it or a similar plan had not been put in practice. He finds,

at times, that he had cases which he would like to send to the hospital, and, at the same time, keep a supervision over them ; but, under existing circumstances, that is impossible. Outside physicians are excluded, yet he did not understand why this should be so, for the plan, where it has been inaugurated, works well ; as, for instance, St. Luke's Hospital, St. Louis, Mo. ; at the Carney Hospital, at Boston ; at Baltimore and in other places. Still there is an objection to allowing physicians to so attend patients. He was of the opinion that much of the objections to such a procedure were among the attending staff of hospitals. There is a disinclination to allow the "outs" to get *in*. The speaker placed himself somewhat in the same position, except that in the paper he expressly stated "general" hospitals. He did not mean to apply that rule to "special" hospitals. He would not recommend that the plan be adopted by the Woman's Hospital any more than by the Eye and Ear Hospital. Still the plan was a good one and worked well wherever adopted ; and he did not see any objection to it on the ground of availability.

DR. WALKER said that, as the committee had undoubtedly looked up the whole subject, he would like Dr. Burge to give the reasons why hospitals had deteriorated in respect of management and control. It was a matter of considerable interest, and should be discussed entire here as a medico-legal question.

DR. BURGE was not aware that deterioration existed ; neither did he know that hospital management had deteriorated.

DR. WALKER did not think that Dr. Burge had answered his question as explicitly as he should. Physicians were responsible for the condition of things. Unfortunately they did not stick up for their rights as medical men. If proper arrangements cannot be made of the character spoken of in this Report, he was of the opinion that medical men were responsible for the failure. But a time will come when such arrangements can be effected ; but they can only be made by members of the profession.

THE CHAIR asked whether the Society was prepared to take any action upon the subject of the Report ?

DR. COLTON moved, and it was so ordered, that the Report of the Committee be accepted and the Committee discharged.

THE CHAIR said that he had omitted to state, in connection with the announcement regarding Dr. Whitney's paper, that Dr. Hunt had prepared a section of the walls of the aneurism for examination under the microscope. He would, therefore, take the liberty of calling upon Dr. Hunt for a demonstration of the same.

DR. HUNT remarked that the specimen had been brought by Dr. Whitney to the Society at its last meeting, but no convenient opportunity had offered for presentation. A few gentlemen, after the meeting, had privately examined the specimen. He could not, from microscopical evidence, say whether it was an aneurism or not. There was no evidence of ossification ; it was simply a mass of fibrous tissue. A section of the degenerated walls of the artery was under the microscope upon a table for inspection ; but there was nothing of interest in it. The case was simply one of calcification of the arterial walls.

THE CHAIR called attention to the fact that there were present several delegates to the American Medical Association, which had just held its annual session in New York ; and asked if they desired to report.

There was no response.

THE CHAIR asked the Committee appointed to draft an obituary of the late Dr. Hamilton, whether they were ready to report.

DR. BURGE remarked that he had been honored with the Chairmanship of that Committee ; but, with the permission of the Society, he would ask that he be allowed to

substitute the name of Dr. Hunt as Chairman, since he (Dr. Hunt) had prepared an obituary notice, and would read the same, if there were no objections.

There were no objections, and DR. HUNT, as Chairman, presented the following report:

Dr. John W. Hamilton was born in Bucks Co., Pa., Nov. 12th, 1819.

After two years spent in Yale College, he entered Jefferson Medical College, Philadelphia, in 1853, and graduated in 1855, after which he spent some time abroad, visiting London, Paris, etc.

Upon his return he settled in Philadelphia, where he enjoyed a good practice until the breaking out of the late civil war. He promptly responded to his country's call and offered his services; was commissioned Acting Assistant Surgeon, U. S. N., on the 21st of November, 1861, with orders to report at once on board the U. S. S. State of Georgia. He was on blockade duty with that vessel until the following October, when he was detached and appointed to U. S. S. Vanderbilt and served upon that vessel during her famous cruise of nearly fifteen months.

Afterwards he served on the ironclad Lehigh, and from this latter vessel he was transferred to the U. S. S. "Maratanza," upon which he remained until the close of the war.

He was present at the fall of Forts Sumpter, Moultrie, Fisher, etc., and honorably discharged August 4th, 1865.

He settled in Brooklyn, May 1st, 1866, where he enjoyed a comfortable practice until about a year before his death, when he was stricken with paralysis and glaucoma, as results of chronic renal disease, which caused his death April 4th, 1880.

Among those who knew him he was noted for his kindness of heart, Christian integrity and abundant charity.

At the East Brooklyn and Bushwick Dispensary, to which he was attending physician from its opening until his death, he was noted for his prompt and faithful attendance.

He was a member of the Medical Society of the County of Kings; the Physicians' Mutual Aid Association; Hilgrove Lodge, F. & A. M., and Rankin Post, G. A. R.

He married, in 1862, Angie W. Wells, daughter of James Wells, Esq., of Norristown, Pa., who with children survives him.

JOS. H. HUNT, M.D.,	} <i>Committee.</i>
J. H. HOBART BURGE, M.D.,	

DR. ALEXANDER HUTCHINS moved, and it was so ordered, that the Report be received, spread upon the minutes and the Committee discharged.

THE CHAIR announced the death of Dr. Nelson S. Drake, and appointed as a Committee to draft a suitable obituary, Drs. A. W. Ford, James Watt and G. Wackerhagen.

THE CHAIR also officially declared Robert T. Davidson, No. 13 Troutman Street, M.D., Bellevue, 1877; P. J. Pendergast, No. 518 Henry Street, Jefferson, 1873, and Thomas Gallaher, 420 Manhattan Ave., M.D., Bellevue, 1879, duly elected members of the Society.

DR. WALKER, after a few preliminary remarks, moved, and it was carried, that the Committee on Hygiene be authorized to expend a sum not to exceed five dollars (\$5.00) for the purpose of experiments in connection with this work, and the Society, at 9.20 P. M., adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρωποισι, κακῶν θελκτῆρ' οδυναῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—ON PAGE 200 will be found a list of odd Journals needed to complete sets. It would be a great advantage to the Library if gentlemen would look over their old journals and send to the Librarian such numbers as are asked for. Or, if information is given to the Librarian where such copies can be had, he will send for them. If it is known where such copies are for sale, that information would be gladly received.

—DR. A. J. LEFFINGWELL, formerly one of our number, contributes to the July *Scribner* a carefully considered paper on vivisection. He presents the arguments *pro* and *con*, but concludes that the operation is employed too often, and with an undue amount of suffering by the animals practiced upon. Physiologists will do well to peruse this article, which is entitled, "Does Vivisection Pay?"

—THOMAS BAYLIS, M.D., died June 15th, at the age of 41 years, at 413 Bergen Street, Brooklyn. He was a graduate in medicine from the University of the City of New York in 1860. He was for many years connected with the Health Department. The fatal illness was Bright's disease of the kidneys.

—NELSON S. DRAKE, M.D., died May 31st, from œdema of the lungs, after an attack lasting twelve hours. Dr. Drake was a native of New Jersey, having been born there in 1831. He had lived in Brooklyn for 29 years. He graduated in medicine from the College of Physicians and Surgeons, New York, in 1853.

—PAYMENT FOR BIRTH REGISTRATION.—Dr. Lyman, of Chicago, proposes that physicians and others, upon whom the laws lay the responsibility of reporting births, shall be paid in bovine vaccine; the number of quills to be proportionate to the number of returns; on the principle that the practitioner who has many births to report will have numerous babies to vaccinate.—*Med. Record*.

—THE NEW EDITION OF WEBSTER'S UNABRIDGED DICTIONARY, with its 1,928 pages, 3,000 engravings, 118,600 words and meanings, including a supplement of over 4,600 new words and meanings, and a new Biographical Dictionary of over 9,700 names—ancient and modern—including many now living), giving the name, pronunciation, nationality, profession and date of each, fully equipped with all available information pertaining to a work of the kind, is one of the most comprehensive books of reference in existence. As a publishing enterprise, having regard to its object, compactness and cheapness, it must be admitted to be a wonderful product of American industry, scholarship and zeal. It reflects great credit upon the enterprise and perseverance of the publishers, Messrs. G. & C. Merriam, of Springfield, Mass., to whom the public are indebted for one of the greatest literary productions of the present century. We heartily commend it to every one of our readers who may desire to become more proficient in the use of the English language.

—THE MEDICAL REGISTER of *New York, New Jersey and Connecticut, for the year commencing June 1st, 1880.*—This necessary appendage to the physician's office appears full of information—fuller than ever before—evincing the laborious, pains-taking industry of its editor and his collaborators. The REGISTER has assuredly won for itself a position of permanence, so that each new year brings anxious inquiry for its re-appearance, and confirms the fullness and trustworthiness of its contents. Each physician in the States represented in the REGISTER should take interest enough in its success to purchase a copy thereof, and, once accustomed to it, and realizing the extent and usefulness of its varied information, very few would fail, in the future, to be on the list of subscribers. The current volume may be obtained of MR. F. B. O'CONNOR, JR., 68 and 70 Court Street, in this city.

—A CURIOUS REMEDY.—A colored woman called on one of our leading physicians yesterday and said she felt bad. Some of her neighbors told her she had this thing, and others told her she had that, but she thought she had “*de risin' ob de lites.*” Continuing, she said her “*lites,*” meaning lungs, “*kept coming up in her throat, and the only antidote that gave her any relief was shot, which she had been taking by the handful.*” The physician told her if shot kept her “*lites*” down, it was the best prescription he could give her, and she left contented.—*Baltimore Herald.*

—CHEAP LITERATURE.—The second edition of Macmillan's Foster's Physiology can be had at 70 Court St. for 75 cents per copy. It is a truly valuable and advanced text-book, with numerous original illustrations.

—THE CORONER SYSTEM ABROAD.—A bill is now before parliament to remedy the defects of the present English coroner system, by securing coroners or medical examiners who are properly qualified by scientific study. During the past forty years there have been many failures in the administration of justice through inefficiency of the present system.* * * A coroner's bill to prevent unnecessary inquests is before the Ontario legislature. The *Canada Lancet* states that it provides that the coroner, before summoning his jury, shall make affidavit before a justice of the peace that he believes the deceased died through violence or unfair means, and that an inquest is needed.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The July meeting will be held on the 20th, at which there will be presented the following papers:

Puerperal Convulsions, by Dr. G. P. Oliver.

Treatment of Cases of Insanity at Home, by Dr. L. C. Gray.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President</i>	C. JEWETT, M.D., 307 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
<i>Secretary</i>	R. M. WYCKOFF, M.D., 532 Clinton Ave.
<i>Assistant-Secretary</i>	J. H. HUNT, M.D., 419 Hart St.
<i>Treasurer</i>	J. R. VANDERVEER, M.D., 301 Carlton Ave.
<i>Librarian</i>	A. HUTCHINS, M.D., 796 De Kalb Ave.

CENSORS.

B. A. Segur, M.D., 281 Henry St.	F. W. Rockwell, M.D., 6 Lafayette Ave.
E. R. Squibb, M.D., 36 Doughty St.	L. S. Pilcher, M.D., 4 Monroe St.
J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK.

(1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI., Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Dr. R. M. Buell
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

AUGUST, 1880.

No. 6.

BLOOD-LETTING IN PUERPERAL CONVULSIONS.

BY G. P. OLIVER, M.D.

A few days after my graduation in medicine, in the month of April, 1851, I was summoned in great haste to attend Mrs. McM—n, an English woman, aged thirty-five years, who was living in the northern part of the city of Philadelphia, Pennsylvania. Her husband came for me and informed me that his wife had been suddenly attacked with "fits." On my arrival at the house I found the woman in bed, in a very small room on the second floor. Ten persons were huddled together in the apartment. Two rough-looking men were on the bed, tightly clenching the upper and lower extremities of the afflicted one. A fussy old matron was endeavoring to pry open the closely-locked jaws of the sufferer with the handle of an iron spoon, to enable her to introduce some common table salt, which, to use her own language, "would break the fit as soon as it touched her tongue." The sick woman was totally unconscious, and deglutition was utterly impossible at this time. I was young and somewhat timid, but inexperienced as I was, I became forcibly impressed with the idea that I had more *help* present than was necessary to bring the case to a successful termination. I at once in a very summary manner dismissed eight of the noisy "*know-alls*" who had been officiating so promiscuously with their hands and tongues. They retired very reluctantly. The husband and mother of the woman remained. The family were

entire strangers to me, and consequently I knew nothing of the patient's history. Whilst I was making inquiries of the husband and mother concerning the previous health and condition of Mrs. McM—n, my attention was attracted to my patient by hearing several successive *snapping sounds*, which occurred almost at the same moment; they reminded me very much of the noise made by the bone returning to the socket, when we reduce a luxation of the hip or the shoulder-joint. I will not attempt to describe, in detail, the terrifying scene that was exhibited at that time, but for the benefit of some of the younger members of our profession who may be with us this evening, I will give a few of the prominent features of the case I had before me.

The countenance of the woman was truly horrible; spasmodic twitchings of the muscles of the left side of the face, convulsive movements of the entire left side of the body, and perfect rigidity of the muscles of the right side; closure of the teeth, and deformed appearance of the lower jaw, it being drawn down on the left side. A large quantity of frothy substance resembling soap-suds, slightly tinged with blood, was being ejected from the nostrils and through the openings of the teeth. A sibilating sound was issuing from the mouth, precisely similar to the hissing of a goose. The blood-vessels of the neck were greatly distended, and were throbbing violently. The face had a turgid and a livid appearance. The eyes were widely open and convergent, giving an exact resemblance of a genuine case of strabismus. The eye-balls seemed to be emerging from their sockets. The head was thrown down on the pillow, and so firmly was it fixed there that I could not command sufficient strength to remove it. The upper and lower extremity of the left side were flexed, whilst those of the right side were extended. The fingers of the left hand were tightly clenched, and those of the right hand were straightened out. The breathing was hurried; the pulse was quick, full and hard during the convulsion, but was scarcely perceptible when the fit had ceased. The convulsion continued about fifteen minutes, and appeared to be most violent about the middle of the paroxysm.

I have given but a faint description of the symptoms of the dreadful malady I had encountered. I had never seen anything like this case before. As I had just stepped into the arena of actual practice but a few days before, and meeting with this formidable foe so soon in my career, I almost hesitated as to what course I should pursue. From the husband and mother I had elicited but little knowledge of the patient's former life. Both husband and mother were not only dreadfully frightened, but they were entirely ignorant of the woman's true condition, although she had been married more than a year. When I suggested that she was *pregnant* they stoutly denied it, and would not listen to me. As

soon as the convulsion had subsided I made an examination per vaginam and found I had in reality a parturient patient, and, as all of the symptoms had shown me, a severe case of puerperal convulsions. In my eagerness to afford relief I never thought of a consultation. I had a fear of blood-letting in this case, as the patient was a spare-looking woman, and I had in my "mind's eye" the loss of blood that would necessarily attend delivery. Chloroform, as an *anæsthetic*, had been in use about four or five years. I had witnessed its potency in a number of surgical cases in the hospital, and I thought it might prove a sovereign remedy in a case of this kind. During an interval of fifteen minutes my patient had remained in a comatose condition, with some little stertorous breathing, and had discharged a small quantity of dark-looking fæces. An examination reveals no dilatation of the os uteri. Another convulsion came on. Chloroform was promptly administered, but it had no permanent good effect; the convulsions were not controlled, and the severe symptoms were but slightly mitigated. The instant the napkin, which was saturated with this powerful agent, was raised from the nose or mouth of the woman, all the direful effects were as visible as ever. For four hours convulsion followed convulsion, each one lasting from ten to fifteen minutes, with an interval of five or ten minutes between each convulsion. During the most of this time she has had bladders filled with ice applied to her head. Injections of oil of turpentine and tincture of asafœtida have been administered by the rectum, and large doses of asafœtida, valerian and other anti-spasmodics have been given internally every half hour. Still there is no abatement of the convulsions. I now have the woman propped up in bed with pillows, and have her held there whilst I open a vein in the right arm; I take away twenty ounces of blood; the convulsion ceased almost at once, and my patient remained perfectly calm for more than two hours. At the end of this time an examination is made; the os uteri has dilated sufficiently to admit the index finger. Another convulsion comes on, but the distortion of the face is less marked. Chloroform is again used, but does not seem to arrest or cut short the convulsions, which continue to come and go every ten or fifteen minutes for nearly two hours longer. During a severe convulsion I again open the vein and take at least sixteen ounces of blood; the convulsion ceases immediately.

From this time forward some improvement in the case is perceptible. Slight labor pains come on every fifteen minutes, and continue for half a minute or more. This state of affairs continued, with little variation, for nearly four hours more, except at times the pains continued a little longer and the convulsions were synchronous with them. Very little if any more dilatation of the os uteri has taken place. The pulse is still quick, hard

and full during the convulsion. I have not left the bedside for twelve hours, and have, during all of that time, closely watched every phase of the case.

As the labor pains become stronger the convulsions seem to decrease in duration. I again open the vein and take eight ounces of blood; the effect is almost like magic; not only does the convulsion instantly subside, but most of the hideous features of the case disappear. The eyes, while they still remain open, have ceased squinting. There is less turgidity of the face and neck, and the lower jaw has assumed a more natural appearance. Instead of the white, frothy substance and the hissing sound from the mouth, we now have a green, biliary matter oozing therefrom, and have low, stertorous breathing. The peculiar *snapping sound*, caused, no doubt, by the action of the tendons upon the bones, is no longer heard at the commencement of the convulsion. Two hours more pass without any very severe symptoms. There is a slight convulsion at the beginning of each labor pain.

Within the next hour a very great change takes place. Labor commences in earnest; the os uteri is rapidly dilating; the bag of waters is protruding with much force. I at once rupture the membrane; an enormous amount of liquor-amnii escapes. I put on my forceps, and in a few minutes effect the delivery of a full-term male child in a state of decomposition.

Immediately after the delivery all symptoms of a spasmodic character ceased. Her face was very pale and her pulse was very weak. She still remained unconscious. There was scarcely any lochial discharge.

During the fifteen hours I was in attendance my patient had sixty-five convulsions. Forty-four ounces of blood were taken from the arm and nearly two ounces of chloroform were used. On account of the extreme debility of the patient, I ordered a very small quantity of wine to be given with water at short intervals. As I was fearful of a return of the convulsions, this course is pursued with a great deal of solicitude on my part. I visited the woman three times during the next twenty-four hours. The color has returned to her face, but she has not spoken. She has had no manifestation of any untoward symptom. More than a pint of thin oatmeal gruel has passed down the œsophagus. Within forty-eight hours the bowels were freely moved without the aid of medicine, and she urinated freely and often during that time.

On the fifth day from the time of delivery, she suddenly started in bed and uttered distinctly, "*William*,"—the name of her husband,—but no other intelligible words came from her lips during the next two weeks. On the twentieth day from the date of her delivery, I was standing near her bed, Mrs. McM—n raised herself in the bed and said, "*Where is*

William P” Her reasoning faculties returned to her instantly. From this time forward she conversed as though nothing out of the usual routine of everyday life had happened. She had not the slightest knowledge of the events of the past three weeks. Within two weeks from this time she was walking in the open air and eating the ordinary food of the table.

It may be proper for me to add here, that twice since this memorable event I have safely delivered Mrs. McM——n, each time of a living child. The first was born about three years after the case I have just related, and the second one about two years later. Nothing unusual occurred in either case. In both cases I had taken the precaution to bleed Mrs. McM——n about twelve ounces one week before each confinement.

Five other cases of puerperal convulsions are all I have ever met with in my own private practice. I have also seen three cases in consultation with other physicians. In one of my own cases, where the patient was only eighteen years old, the convulsions did not occur until four hours after the delivery of a live child. In this case I took but eight ounces of blood from the arm; used leeches on the temples; cold applications to the head and anti-spasmodics internally. She had but six convulsions and was well within two weeks. This case occurred during the year 1859.

In the year 1870 another one of my own cases was treated. She was a fleshy woman, twenty-five years old. In this case, during the interval between the paroxysms, the patient conversed rationally. After I had used more than half an ounce of chloroform, and had wasted more than two hours of time, I opened a vein in the arm and took eight ounces of blood, which modified the symptoms wonderfully, but did not entirely check the convulsions. At the end of two hours I opened the vein again and took eight ounces more of blood. I gave anti-spasmodics; used cold applications to the head. She had thirteen convulsions. I finally delivered her of a dead child with the forceps. The convulsions ceased at once. Within three weeks she entirely recovered. The other three cases occurring in my own practice were as follows:

An unmarried girl, sixteen years of age, robust and healthy in her appearance. She had but five convulsions. I abstracted about twenty ounces of blood. The labor was completed in less than three hours. The child was dead. The patient was entirely well within two weeks. This case occurred in the spring of 1874. In the fall of the year 1876 I delivered this same young woman of a living male child, and she had no symptoms of a spasmodic character and the labor was concluded in one hour and twenty minutes from the commencement of the first labor-

pain. Of the other cases I shall say but little, as I do not wish to occupy the time of the Society with rehearsing the incidents of a number of cases having no distinctive feature that will make them more interesting than those already detailed.

It may be sufficient for me to say that the two remaining cases of my own were aged—one, twenty-three years, the other twenty-seven years. The younger one had nine convulsions. Thirty-two ounces of blood were abstracted and the labor lasted five hours. The older one had six convulsions; sixteen ounces of blood were taken and the labor continued ten hours. Both children were dead. All of my own cases, and the three I saw in consultation, nine altogether, were all *primiparæ*, and in seven cases, where the convulsions came on before the delivery, the children were dead. In the two cases, where the convulsions followed the delivery, both of the children lived. All of these women were of fair complexion, with light hair and blue or gray eyes, and were of a nervo-sanguineous temperament. As far as I could learn, neither of them had been subject to epilepsy, catalepsy or spasms of any kind in earlier life. I believe they are all alive at this time. One or two facts I wish to impress. All of these cases were *bled*, and all of them *recovered*, most of them rapidly.

In preparing this paper I have not said anything concerning the ætiology of the disease, as that discussion and the citation of the opinions of the various authors who have written on the subject are matters I shall not bring forward at this time. My desire is merely to give a brief history of the few cases it has been my lot to treat during the past thirty years of my medical life. From my own experience and from information I have gathered from others, I am convinced that *venesection* is the only safeguard we possess at the present time that will speedily arrest the evil effects of eclampsia partura. I regard it as the *sheet-anchor* of the profession in checking this dreadful disease. It should be performed at the very onset of the attack, by abstracting at least twelve or fourteen ounces of blood at the first opening of the vein, and should be fearlessly repeated in severe cases two or three times, or until its good effects are fully realized in the case.

I am very sorry to say that, although but a few years ago the medical profession were almost unanimous as to the necessity of blood-letting in puerperal convulsions, yet I am led to believe, at this time, many good physicians, owing to a distaste manifested by some of their patients to this mode of treatment, have, in their endeavors to please the patient, or the family, in many instances, so far indulged this prejudice as to seriously compromise their own self-respect and produce upon the patient the most disastrous consequences. If this is so, it becomes a matter of vital im-

portance for physicians to act with decision, and to allow no "pragmatic prattler" to interfere with any of their known duties.

I have in this paper but reiterated that which has been written a hundred times before—"the necessity of blood-letting in puerperal convulsions." But if the repetition here will make the impression desired, and the meed of praise so often bestowed upon this treatment by some of the brightest medical lights in the world will have the effect of fostering in the minds of the members of our profession a disposition to test its efficacy, I will then feel that I have not labored in vain.

Since writing this article I found in the Journal of the Proceedings of our Society an article, published in the May number of 1879, on Puerperal Convulsions, contributed by our estimable Assistant Secretary, Doctor J. H. Hunt. Although the doctor tells us he attributes his success in this case to the use of jaborandi, yet, if we scan his treatment carefully, we will find that the other very excellent remedies used had very much to do in effecting the pleasing termination of this interesting case. I fear that our materia medica has not yet furnished a *specific* for this malady. I, however, must confess that I am so little acquainted with jaborandi, owing, perhaps, to the dread I have conceived of it after reading the experience of Professor Fordyce Barker, published in the Transactions of the Medical Society of the State of New York, in 1879, and as I have seen but one case of the disease since I heard of this remedy, I do not think myself competent to speak either *for* or *against* its use in the treatment of this disease.

DISCUSSION.

DR. SANDFORD had seen several cases of puerperal convulsions, but since the reader of the paper had given such wonderful results from blood-letting, it was proper for him to say that he did not think that this procedure always gave such good results, and that there are other remedies which produce quite as good effects.

In his first case blood was drawn freely and rapidly. He did not remember the exact quantity, but the practice was most heroic. The convulsions were only temporarily controlled; the remedy had absolutely no permanent effect. It did not do even what chloroform would do. Delivery was not accomplished by the blood-letting. Dilatation of the os was so slight that it was deemed best not to attempt delivery; although in the light of subsequent reading and experience, he was of the opinion that the os was in such a condition that it would now be considered criminal not to have proceeded to deliver with the aid of the forceps. She died after heroic blood-letting.

The second case which had come under his notice was that of a healthy primipara, who was taken in convulsions just after a perfectly normal labor, and after giving birth to a healthy child. The convulsions were severe and constant—there was no interval in which the patient became at all conscious. Chloroform was used at once and followed by a procedure which was once strongly recommended in the journals, namely, large doses of the bromide of potassium and hydrate of chloral. These remedies

seemed to have a wonderful effect, so much so, that it satisfied him that they acted as a controlling element. The patient made a safe and rapid recovery.

These two cases, although few in number, served to teach that physicians should not be guided entirely by the results of single cases; and it seemed to him that the author of the paper had gone a little too far in some of his deductions and expressions, for instance, when he says that delay in blood-letting was a bad thing, a waste of time, etc. He did not consider it a waste of time when chloroform, bromide and hydrate of chloral were used in the second case. The effect of chloroform, and especially of chloral and bromide following chloroform, was very apparent, while in the first case, blood-letting certainly had no good effect. The doctors bled continuously from a sense of duty, for they had been educated in that practice, while the speaker had been educated in the age of chloroform, which latter agent, in his opinion, is of much greater value in controlling convulsions.

DR. HUTCHINS had had no experience in the use of the lancet in the treatment of puerperal convulsions; but, inasmuch as the author had referred to the fact that the practice of blood-letting, up to within a few years, was in great favor, he would take the liberty of reading from a journal which had just arrived (*Half-Yearly Compendium of Medical Sciences*, July, 1880) a few extracts relating to the question in hand: (The reader may consult "Puerperal Convulsions," a Lecture, etc., by R. T. Coleman, M.D., Prof. of Obstetrics, Med. Col. of Va., Richmond. *Va. Med. Monthly*, March, 1880.)

DR. OLIVER here stated that he had no idea when he wrote his paper that such an article was in existence. His paper gave his own deductions from his study of cases occurring in his own practice.

DR. SEGUR related the following case: In the seventh month of her pregnancy his patient began to be anasarcaous. A month after that she returned to this city—she had been in the country—and at the beginning of the last month came under his observation. She then had a large amount of albumen in the urine, with hyaline and granular casts. She suffered a great deal from headache, and he was in constant apprehension lest she might develop convulsions before her term. Very frequent examinations of the urine were made for urea, and the quantity passed daily was a little less than one-half the normal standard. The amount of urine passed was about sixteen to twenty ounces (daily.)

During the month preceding the end of gestation, the treatment consisted in the use of free purgations—the bowels being moved freely by jalap and cream of tartar, about 20 grs. jalap and 3 drachms cream of tartar, which was followed by three or four watery evacuations, with considerable relief to the pain in the head. She would go on for three or four days very comfortably; then there would be a return of the disagreeable symptoms, when free purgation would be again resorted to with good results; and so the case would continue. He also administered small doses of belladonna continuously. In that way she passed along up to the end of her term, when she fell in labor, and everything went on as usual until the head of the child was pressing on the perineum and passed into the world; simultaneously with that event she went into a convulsion which continued until after the child was born and she was delivered of the placenta. In about half an hour after this she became sensible, but she could not see; she was, for the time being, blind. In the course of about an hour after the first convulsion a second one occurred, and it was decided, in view of the fact that she had been copiously purged during the month previous to her confinement, that it would be prudent to use morphine injections for the relief of her convulsions. Therefore, after the second convulsion, a hypodermic injection of morphia was given. After this, a third convulsion occurred about half an hour later, when another hypo-

dermic injection of morphia was given, and a blister was applied to the back of the neck. She had no further convulsions and passed a comfortable succeeding day, and in about twenty-four hours after the last convulsion she recovered her sight, and in the course of about two months the albumen had entirely disappeared from the urine, and she has enjoyed good health since.

In reflecting upon the foregoing case, and after reading upon the subject, Dr. Segur had come to the conclusion that the previous purgation prepared the way for the safe use of the hypodermic injections of morphia; and the favorable effects of the morphia, as well as the happy result of the case, he had accounted for in his own mind by supposing that in such cases morphia could be more safely administered than it could be where the patient, in a plethoric condition, had fallen into convulsions after previous treatment.

DR. GRAY considered that there was another and entirely different standpoint from which this matter of puerperal convulsions could be studied. It is, of course, very difficult and unfair to question any man's individual experience as to the efficacy of blood-letting for the relief of puerperal convulsions. The question which we are bound to consider is, what is the best remedy wherewith to control these convulsions? Is blood-letting better than any other remedies, or is it the only remedy that will answer the purpose? A woman who is in the puerperal condition is certainly not endowed with exuberant strength, and she has neither blood nor energy to lose; and if we can accomplish our purposes by the administration of bromide or chloral, which can be so employed as to have a sedative effect while controlling the convulsion, it is much wiser to use them than to take away that blood which she can so ill spare. In those cases, too, where the convulsions are due to albuminuria, there already exists a condition of anæmia, and in that condition she can hardly afford to lose her blood.

While it is an undoubted fact that blood-letting has its uses, as in a great variety of instances in controlling pain and nervous action, still the tendency of the profession at the present day is not to use it, but rather to use the bromides, chloral, veratrum viride, and other remedies. Therefore, it seemed to him, blood-letting should be used when the same effect cannot be gained by other less dangerous remedies.

DR. SHERWELL has employed blood-letting in the treatment of puerperal convulsions. In one case he drew a quart of blood, nearly. She never had another convulsion. He also gave, in this case, a hypodermic injection of veratrum viride—supposed to be, as far as he knew, the first instance in which this remedy was employed as an injection. The quantity employed was five minims. That was some time before veratrum viride was employed as an injection hypodermically by Buckley. In this case both mother and child did well. The urine was almost solid with albumen, which passed away in due time.

DR. IRISH saw a case of puerperal convulsions of great severity. The os uteri was dilated and immediate delivery performed. She had had before delivery seven spasms, and they continued after delivery. Bleeding was resorted to—twenty ounces being drawn from the left arm, with no benefit. Convulsions followed each other in rapid succession. Chloroform, bromide and chloral were given, but the patient died. The child was born, dead. The previous history of the patient could not be ascertained.

DR. HUNT remarked that he had seen in consultation and otherwise some cases of puerperal convulsions. In all but one, chloroform, that most valuable of adjuvants in the treatment of convulsions, was used freely. In the first two cases, veratrum viride was the drug used in addition to the chloroform, and in both cases the convulsions ceased as soon as the patient was fairly under the influence of the drug, which was used hypodermically.

In another case, where the convulsions kept up after the delivery of a partially decomposed child, fl. ext. of senna was given in full doses, repeated as the patient could swallow, and in this case the convulsions ceased and the patient's mind became clear as soon as the purgative effect of the senna was obtained.

In one case, where he was called in to a large, plethoric woman, who was nearly at full term, he found her in a violent convulsion, and attempted to bleed her while the husband went for chloroform; he had succeeded in getting but about two ounces of blood, when it ceased to flow; the woman was dead. She died during her first convulsion, which was one of unusual severity.

In two cases, one of his own and the other with a brother physician, bromide and chloral were the agents depended on in addition to the chloroform; in both, the convulsions kept up after delivery, one case yielding after about six hours, and the other kept up for over three days, and though large amounts of chloroform and chloral were used she died.

Two cases he had treated with jaborandi, one of which is described in the volume of THE PROCEEDINGS for 1879, and one since then. Both were markedly uræmic, both kept up after the birth of the child, both yielded promptly to the profuse diaphoresis of the jaborandi, and in neither did he have the extreme prostration so much dreaded by Prof. Barker and others.

In all these cases, except the woman who died during her first paroxysm, the convulsions kept up after delivery, notwithstanding there was large amount of blood lost during the forcible and rapid delivery in each case.

It seemed to the doctor that the proper thing to do in these cases was to relieve the system by one of the natural emunctories. The kidneys seemed to be unavailable, for the reason that we had no diuretic which we could depend upon to act with the necessary promptness, and we must therefore depend on the bowels and skin. He felt that we were in no more danger of producing prostration by the use of jaborandi than we were by excessive blood-letting.

DR. MATTISON referred to a case of puerperal convulsions in his own family which was treated successfully by morphia and blood-letting.

He had always considered the bleeding as an important element in the successful treatment of the case. There was one important feature in the case, and this was the effect of morphia upon the child, which for some hours after birth, appeared lifeless. It, however, recovered, but only after most vigorous efforts by the employment of artificial respiration, etc. It was evidently narcotized.

This case is fully reported in the *Medical and Surgical Reporter* for Feb. 7th, 1877.

DR. SULLIVAN had seen thirteen cases of puerperal convulsions. Were he called to a case of convulsions occurring before delivery, he would resort to blood-letting; if after delivery, he would depend upon morphia.

DR. SEGUR inquired of Dr. Sullivan as to the rate of mortality in his cases.

DR. SULLIVAN replied that he thought one-half the number died. He did not believe in morphia before delivery, but he had noticed in a recent article in the *American Journal of Obstetrics* that morphia is the treatment at any time.

DR. SEGUR asked the reader of the paper if he could tell the cause of death of the child—whether it died during the convulsion or whether it died before or after the convulsion.

DR. OLIVER replied, of the seven cases, three children were decomposed when born. He did not know that he could give the cause of death. They undoubtedly died before the convulsions.

ON THE TREATMENT OF CERTAIN CASES OF INSANITY AT HOME.

BY L. C. GRAY, M.D.

One of the defects of our profession in America is that the general practitioner has not yet been stimulated, by current thought or individual reflection, to gain such a knowledge of insanity as he possesses in regard to other special topics, and as is usually possessed by the physician of Continental Europe. The subject is more absolutely relegated to the specialist than is dentistry to-day, or than were diseases of the eye and ear a few years ago. This indifference to the matter frequently results in peculiar misfortune to the patient. No harm can come from placing a person in charge of a dentist, or aurist, or oculist, even though the treatment might have been as well conducted by the general practitioner; but when an insane person is unnecessarily committed to an asylum a great wrong has been done. Aside from the distrust with which the individual will always be regarded thereafter, and which will place serious obstacles in his career in many walks of life, the mental effect of the confinement, the greater because often unconscious, and still the greater because of the inherent tendencies of the patient's mind, must also be carefully considered. Moreover, it too often happens that the treatment, as conducted in the asylums, is of little benefit to the patient, or is positively injurious. Any one who is familiar with the county and municipal asylums is also familiar with the over-crowded wards, in which patients are herded together like sheep, the maniac by the side of the melancholic, the convalescent cheek-by-jowl with the hopelessly incurable, and where the sufferers are left to wear out their ailment or their life without aid from medicine, steady occupation, or out-door exercise. It is possible that there are some who may deem this assertion exaggerated. To all such I can only say that if they will visit the institutions around New York, they will then not think my general statement unjust or incorrect. Private asylums are usually possessed of better and more pleasing surroundings, but it is a lamentable fact that in them, too, medicinal treatment and systematic occupation are too much neglected, and the patient is committed to the care of that *vis medicatrix naturæ* which is so much prated about, and whose exact capabilities are only understood by the most careful, experienced and thoughtful therapeutists. To this rule there are, of course, many distinguished exceptions. The expense of private asylums furthermore, places them beyond the reach of people of moderate means. For these reasons it is peculiarly the duty of the physician to pause before

committing a patient to the asylum, and to consider whether there are not insane persons who can be better treated at home.

There are many such. First and foremost among them are the cases of melancholia, a large proportion of whom can be readily taken care of in this manner; indeed, I treat patients of this class at my clinic, usually with favorable results. The psychoses of pregnancy and the puerperal state are usually manageable by the family physician—a fact which medical men largely appreciate, although by no means to the proper extent. Finally, there are a number of miscellaneous cases of insanity which are characterized by mental excitement, which come on suddenly, which are either due to appreciable and remediable causes, or which occur in persons who seem otherwise healthy, and which can be relieved by removing the cause and subduing the excitement. It is, of course, useless to expect any more than temporary relief in such grave diseases as general paresis, paralytic dementia, inveterate epileptic insanity, acute mania, or whenever the mental impairment is of such a nature as to peremptorily need the special advantages of an asylum.

The treatment of this class of cases cannot be more than outlined in a short paper of this kind, whose object it is rather to call attention to the subject than to dwell upon it at length and with elaboration. There are many little details which are simple matters of experience, and cannot be explained any more than can similar facts in other departments of therapeutics, and which may yet impart a special complexion to each group of cases. These must necessarily be omitted. But before entering upon what I am about to say in a somewhat general manner, let me warn the physician to disabuse his mind of a false idea, which he half recognizes as false, and which yet so largely influences his thoughts and his actions, viz., that insanity is a disease apart, beyond the reach of drugs. Insanity is a disease of tissue, a disease of the cortex of the brain, as distinctively as pneumonia is a disease of the lungs, or as nephritis is of the kidneys, and is just as responsive to the proper remedies as are those affections.

In all varieties of mental derangement it is quite as important as in other maladies, if not more so, to search for disorders of any of the other organs, and relieve them as far as possible. Happy, often astonishing results, will sometimes ensue from attention to this point. Marked hallucinations, for example, have been cured by the removal of impacted wax from the ear; and I have a patient who at long intervals becomes insane and at the same time dysmenorrhœic, and who has twice been cured by the cure of her dysmenorrhœa. The melancholics need stimulation to a greater extent than any of the other insane; indeed, Meynert speaks of them as cases of cerebral exhaustion par excellence. They must be

given quinine and cod-liver oil, iron if there be anæmia, and phosphorus, which can be added to the emulsion of the cod-liver oil in the form of Thompson's solution, a drachm at a dose. Opium should be given, preferably as sulphate or bimeconate of morphia, the latter preparation being much the least liable to induce constipation or any other unpleasant effect. The dose should be at the beginning, gr. 1-6 twice a day, and it should be increased with a free hand as the patient becomes tolerant of it, reaching as much, if necessary, as a grain at a dose. No fear need be had of the formation of the opium habit. The diet must be generous, and alcoholic stimulants liberally administered. The treatment of the pregnant and nursing insane will vary somewhat according to the symptoms. There are two important generalizations, however, that must always be clearly kept before the mind concerning these women. In the first place, pregnancy and lactation are, as a rule, positively deleterious to them. The physician must therefore carefully consider whether the requirements of the case are such as to make it necessary to empty the uterus, wean the child, or for the future prevent further pregnancies. In the second place, these patients generally require supporting treatment; and quinine, alcoholic stimulants and as generous diet as the peculiar condition will permit must be used freely. Sedatives should be carefully employed. The most efficient of them is the one which has been most recently introduced to the profession, viz.: hyoscyamine. Where there is mental excitement, especially when it is violent, Merck's extract of hyoscyamine should be given. I prefer the crystallized extract, because I have learned that it is most efficacious and least unpleasant in its effects. One grain can be put in solution in 50 minims of alcohol, one drop containing, therefore, gr. 1-50. This dose should be given, either by the mouth or hypodermically, every three or four hours, until the patient passes fully under its influence and becomes calm. The dose of the uncrystallized extract is gr. 1-12 at the same intervals. The ordinary preparations of hyoscyamus are utterly worthless for this purpose. The dose must be increased from time to time, so as to keep the patient quiet. There is no danger to be anticipated, unless the patient is very much debilitated. I have given the drug to the insane continuously and in increasing doses for over thirty days, without the supervention of any constant depression of heart or respiration. It is a curious and inexplicable fact, to which I have called attention in a late paper, read before the American Neurological Association, that quinine, given in full doses with hyoscyamine, increases the calmative effect of the latter drug upon the mental symptoms, at the same time that it removes or decreases the listlessness and languor which the hyoscyamine induces. Hyoscyamine—it has been felicitously termed a "chem-

ical restraint"—is of particular value in the miscellaneous cases of mental excitement to which I have alluded. The patient must be fully brought under its influence, and, not infrequently, when this is done, and the patient is kept quiet, sanity will be restored. The utility of the quinine should not be forgotten, and it should be administered in cases that are in need of tonics and stimulation. But hyoscyamine cannot be relied upon to produce sleep, and, for this purpose, it is best to employ the bromide of potassium or sodium and the hydrate of chloral. The bromides are best adapted to sthenic individuals, whilst the chloral answers better in debilitated persons; but the combination of a bromide and chloral meets most indications. The surroundings should be made as quiet and pleasant as possible. Melancholics must be cheered up, spurred on to efforts to overcome their gloomy thoughts—they must, in a word, be *dominated*; friends must be carefully instructed to encourage them briskly; and, whilst the patience of physicians and patients will be severely taxed, success will be an ample reward. In cases of mental excitement the patient must be judiciously humored, and guided rather than led. Upon no occasion whatsoever, short of absolute violence, should force be used or threatened. Indeed, there is no situation in life in which firmness, tempered with tact and the kindness of a good judgment, is more telling than in the treatment of the insane.

DISCUSSION.

DR. MATTISON referred to Dr. Gray's statement that hyoscyamine could not be relied upon to produce sleep. The speaker had used this remedy in many cases with beneficial effect. It is used not only in cases of insanity, but under other circumstances, quite extensively. Its effect is to produce sleep of long duration, in some cases from twelve to fourteen or sixteen hours, if given in grain doses. Smaller repeated doses will cause excessive excitement. Dr. Gray's experience does not, in the opinion of Dr. Mattison, accord with the views of writers on the subject.

DR. GRAY remarked that sleeplessness is by no means a simple condition. A person may be sleepless because on the one hand he has a congested brain, and, on the other, because he is anæmic, and thirdly, because he has not enough food to eat, while still a fourth cause may be because he is in a state of maniacal excitement. Now, in a certain proportion of all these cases, if you remove the underlying cause, the congestion, the anæmia, the starvation or the maniacal excitement, you will get sleep, while the remainder, notwithstanding the removal of the above-mentioned causes, will remain sleepless still. Now hyoscyamine, when the patient is laboring under excitement alone, will remove that; and if the removal of that cause will enable the patient to go to sleep, hyoscyamine will act as a hypnotic. But in the majority of cases in his experience the patient requires something more to induce sleep, after the mental excitement is removed; and this is where the bromides and the chloral come into play. He had treated a large number of cases at the Flatbush Lunatic Asylum, at his clinique and in his own private practice; and he had been careful to limit his statement by say-

ing that hyoscyamine could not be relied upon, and this was his conscientious opinion, after a careful analysis of his cases. Some time ago he prepared a pill, containing one-tenth of a grain of hyoscyamine. The effect was to produce great excitement. It was almost impossible to dispossess the patient of the idea that he was going to die. An emetic was given, but it was impossible to get that patient to sleep for two nights after; and that is a very excellent illustration of the worthlessness in a large proportion of cases of hyoscyamine as a hypnotic.

DR. MATTISON replied that Dr. Gray's experience with one-tenth grain doses of hyoscyamine exactly coincided with his own, and was exactly the same as with one-eighth grain doses. The question is, whether full doses of hyoscyamine would not have produced the desired effect—namely, sleep. Larger doses, as one grain, produce good results. The experience of the profession with this drug may be the same as that with nitrite of amyl and other drugs. While small doses may produce unsatisfactory results, larger doses yield better results.

DR. GRAY said that it seemed to him that in giving these extremely large doses of hyoscyamine and other drugs, sleep might be induced by the production of narcotism. Sleep can be produced by almost any drug; by morphia, or even by water; and if enough hyoscyamine is given we can produce coma—actual, downright narcotism. Of course a man will go to sleep in that condition. He had given this drug in extremely large doses (he was not prepared to say how large, from memory), but both himself and Dr. Shaw, of the Flatbush Asylum, had satisfied themselves that they had not been able to rely upon the hypnotic effects of the drug.

AN ACT ENTITLED "AN ACT TO REGULATE THE LICENSING OF PHYSICIANS AND SURGEONS."

PASSED MAY 29, 1880; THREE-FIFTHS BEING PRESENT.

*The People of the State of New York, represented in Senate and Assembly,
do enact as follows:*

SECTION 1. A person shall not practice physic or surgery within the state unless he is twenty-one years of age, and either has been heretofore authorized so to do, pursuant to the laws in force at the time of his authorization, or is hereafter authorized so to do as prescribed by chapter seven hundred and forty-six of the laws of eighteen hundred and seventy-two, or by subsequent sections of this act.

§ 2. Every person now lawfully engaged in the practice of physic and surgery within the state shall, on or before the first day of October, eighteen hundred and eighty, and every person hereafter duly authorized to practice physic and surgery shall, before commencing to practice, register in the clerk's office of the county where he is practicing, or intends to commence the practice of physic and surgery, in a book to be kept by

said clerk, his name, residence and place of birth, together with his authority for so practicing physic and surgery as prescribed in this act. The person so registering shall subscribe and verify by oath or affirmation, before a person duly qualified to administer oaths under the laws of the state, an affidavit containing such facts, and whether such authority is by diploma or license, and the date of the same and by whom granted, which, if willfully false, shall subject the affiant to conviction and punishment for perjury. The county clerk to receive a fee of twenty-five cents for such registration, to be paid by the person so registering.

§ 3. A person who violates either of the two preceding sections of this act, or who shall practice physic or surgery under cover of a diploma illegally obtained, shall be deemed to be guilty of a misdemeanor, and on conviction shall be punished by a fine of not less than fifty dollars nor more than two hundred dollars for the first offense, and for each subsequent offense by a fine of not less than one hundred dollars nor more than five hundred dollars, or by imprisonment for not less than thirty days nor more than ninety days, or both. The fine when collected shall be paid, the one-half to the person or corporation making the complaint, the other half into the county treasury.

§ 4. A person coming to the state from without the state may be licensed to practice physic and surgery, or either, within the state, in the following manner: If he has a diploma conferring upon him the degree of doctor of medicine, issued by an incorporated university, medical college or medical school without the state, he shall exhibit the same to the faculty of some incorporated medical college or medical school of this state, with satisfactory evidence of his good moral character, and such other evidence, if any, of his qualifications as a physician or surgeon, as said faculty may require. If his diploma and qualifications are approved by them, then they shall indorse said diploma, which shall make it for the purpose of his license to practice medicine and surgery within this state the same as if issued by them. The applicant shall pay to the dean of said faculty the sum of twenty dollars for such examination and indorsement. This indorsed diploma shall authorize him to practice physic and surgery within the state upon his complying with the provisions of section two of this act.

§ 5. The degree of doctor of medicine lawfully conferred by any incorporated medical college or university in this state shall be a license to practice physic and surgery within the state after the person to whom it is granted shall have complied with section two of this act.

§ 6. Nothing in this act shall apply to commissioned medical officers of the United States army or navy, or of the United States marine hospital service. Nor shall it apply to any person who has practiced medicine

and surgery for ten years last past, and who is now pursuing the study of medicine and surgery in any legally incorporated medical college within this state, and who shall graduate from and receive a diploma within two years from the passage of this act.

§ 7. All acts or parts of acts inconsistent with the provisions of this act are hereby repealed.

STATE OF NEW YORK, }
Office of the Secretary of State. } ss. :

I have compared the preceding with the original law on file in this office, and do hereby certify that the same is a correct transcript therefrom and of the whole of said original law.

JOSEPH B. CARR, *Secretary of State.*

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A regular stated meeting of the Medical Society of the County of Kings was held at 398 Fulton Street, Brooklyn, on the evening of Tuesday, July 20th, 1880, the President, Dr. Charles Jewett, in the chair, Dr. J. H. Hunt, Assistant Secretary, acting as Secretary.

The meeting was called to order at 8:22, with about 30 members present.

The minutes of the previous meeting were read and approved.

THE PRESIDENT announced the substitution of Dr. Moore for Dr. Ford upon the Committee to prepare a proper obituary relative to the death of Dr. Drake, and the appointment of Dr. Watt as Chairman.

DR. WATT, Chairman, then read the following report, which was received, ordered on file and this Committee discharged.

DR. NELSON SAMUEL DRAKE was born in Plainfield, N. J., June 28th, 1831. After receiving a common school education he entered Union College, at Schenectady, where he remained for two years ; he then returned home and began the study of medicine, in the office of Dr. Charles H. Stillman, who is still living and practicing at Plainfield. He matriculated at the Vermont Medical College in Woodstock, graduated in 1852, then came to New York City and received an ad eundem degree from the College of Physicians and Surgeons, 1853 ; he commenced the practice of his profession in this city, in 1854, with a brilliant success, until the breaking out of the civil war, when he was appointed surgeon to the transport steamer Daniel Webster, also served as surgeon to the 16th Cavalry, also as surgeon of the Reserve Artillery, under General Hayes, at Pittsburgh Landing.

He returned to this city in 1865, again to resume practice. He was an extensive reader of medical works, and, with a wonderful retentive mind, he was what might well be called a walking "Medical Cyclopædia."

He was a member of Mistletoe Lodge, F. & A. M., of which he was Master for two years, a member of the Kings County Medical Society and of the Reformed Church.

He died after a short illness on May 31st, 1880, from pulmonary œdema, leaving a wife and one son.

JAMES WATT, M.D.,
GEO WACKERHAGEN, M.D., } *Committee.*
J. FRED. MOORE, M.D., }

The following propositions for membership were made :

L. T. Bell, M.D., Bedford Avenue, near Putnam, L. I. C. Hospital ; proposed by Dr. Jewett ; William D. Spencer, M.D., Col. P. and S., N. Y., 1868, 384 Court Street ; Joseph E. Smith, M.D., Jefferson, 1875, 322 S. 5th Street (E. D.) ; proposed by Dr. Russell ; Edward W. Owen, M.D., 541 Grand Street (E. D.) ; by Dr. Hardrich ; G. A. Schwartzenberg, M.D., 292 S. 5th Street (E. D.) ; by Dr. Baker ; George W. Wells, M.D., Bell. H. M. C., 1868, 307 Clinton Street ; by Dr. Segur.

THE ACTING SECRETARY read the minutes of a special meeting of the Society, as follows :

"A special meeting of the Society was held at 413 Bergen Street, June 18th, at 3:55 P. M. About twelve members present.

"On motion of DR. MANN, it was *Resolved* : That the members present attend the funeral of Dr. Baylis in a body. Adjourned."

On motion, the minutes of the special meeting were adopted.

THE COUNCIL recommended the acceptance to membership of the following duly nominated candidates :

A. G. Meyersburg, M.D., Univ. of the City of N. Y., 1875 ; S. J. Sørensen, M.D., Univ. Copenhagen, 1871 ; G. A. Cardwell, M.D., Col. P. and S., N. Y., 1880.

THE CHAIR stated that the Treasurer desires the attention of members to be directed to a note which appears upon the card, namely : "The Treasurer will be present at the meeting to receive unpaid dues." Being unavoidably absent, however, he desired also that some one should be requested to act as his substitute for the time being. In accordance with this, the Chair appointed Dr. Benj. Edson as such substitute.

The Society then listened to the first paper of the evening, entitled, "Puerperal Convulsions," by GEORGE P. OLIVER, M.D.

It was debated by Dr. Sandford, Dr. Hutchins, Dr. Segur, Dr. Gray, Dr. Sherwell, Dr. Irish, Dr. Hunt, Dr. Mattison, Dr. Sullivan and the author.

DR. LANDON C. GRAY read the second paper, entitled, "Treatment of Certain Cases of Insanity at Home," which was briefly discussed by Dr. Mattison and Dr. Gray.

THE CHAIR appointed as a Committee to prepare a suitable obituary in reference to the death of Dr. Thomas Baylis, Dr. George K. Smith, Dr. J. H. Raymond and Dr. S. E. Fuller.

THE CHAIR called attention to the fact that the last legislature had enacted a Registry Law. This law requires every practitioner of medicine and surgery to register his name with the Clerk of the County before October next, and imposes several other requirements, at the same time attaching penalties for the non-observance of its provisions.

In view, therefore, of these facts, and of its obvious importance to members of the Society, he suggested that a copy thereof should be incorporated in THE PROCEEDINGS.

On motion of DR. SULLIVAN, it was so ordered.

THE CHAIR announced that the following named gentlemen were duly accepted to membership in the Society :

Augustus F. W. Reimer, M.D., Göttingen, 1860 ; George McNaughton, M.D., Bellevue Hosp. Med. Col., 1878 ; Wm. Waterworth, M.D., Bellevue Hosp. Med. Col., 1878 ; Melville W. Barnhart, M.D., Univ. of N. Y., 1879 ; Alfred S. Houghton, M.D., Col. P. and S., N. Y., 1880 ; Charles Henry Wilson, M.D., Univ. of N. Y., 1876 ; John A. Arnold, M.D., Bellevue Hosp. Med. Col., 1871, and the Society, at 10:14, adjourned.

Ἀσκληπιὸς.



ὁ Σωτηρ

Χάρμα μέγ' ἀνθρώποισι, κακῶν θελκτῆρ' οδυναῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—THE LIBRARY.—The committee appointed at the last annual meeting to canvass the city on behalf of the Library reported progress to the Council at its late meeting, and formally handed over to the Librarian the results of its labors thus far. A magnificent collection of the latest contributions to medical literature is now upon the Library shelves and has already been consulted by many members of the Society. A still further addition will be made in a few weeks, so soon as it can be received from England. The books have each the impress of the Society's stamp and are free to all members of the profession. They cannot, of course, be removed from the Rooms. The committee has yet on hand some funds with which to make further purchases, and would be glad to receive suggestions as to any desirable books. A full catalogue will be furnished with the final report of the committee.

—AN IMPORTANT ADDITION to the Library has lately been made in the bound volumes of the more prominent journals. A few additions are needful to make the sets complete. Considerable attention has been given to the request in the last number of THE PROCEEDINGS (and repeated on page 221 of this number) notably by Drs. CHAPMAN and VROGMAN, who have made some very valuable contributions and enabled the completion of some very important files. Old and odd numbers of journals, now so much rubbish on the shelves in many physicians' offices, if sent to the Rooms may serve an excellent purpose in completing files or as matters of exchange. Capt. Charters is authorized to pay the express charges on any such packages that may be sent to the rooms. It may be suggested that files of journals are rarely referred to by any one individual, and it would be a generous act for the owners of such files to donate them to the Library, where they can be accessible to the many.

—ANNUAL DUES.—The Treasurer desires to call the attention of members of the Society to the fact that the Annual Dues are COLLECTIBLE AT THE BEGINNING OF THE YEAR. Payments have been made very slowly thus far, and the money is needed to defray the running expenses of the Society. He will be greatly obliged if the members of the Society would forward to him at his residence, 301 Carlton Avenue, or if they would respond promptly to the call of the collector.

—MR. SEYMOUR HADEN, of London, is an artist as well as surgeon. He is rated as one of the first, if not the very first, of living etchers. *Scribner's* for August contains an illustrated article that will be welcome to many physicians who have been wishful to know more about the achievements of Mr. Haden.

—EXTEMPORIZED SALICYLATE OF CALCIUM IN THE SEROUS DIARRHEAS OF INFANTS.—A paper on this topic will be presented at the August meeting of the Society. The treatment, which is new, has been sufficiently effective to justify an advance notice of the paper, that it may have further trial during the season when these diseases are especially prevalent. Any experience, favorable or otherwise, if presented at that meeting, will help to define the scope of its usefulness. A couple of formulas are subjoined :

R. Acid Salicylic, parts, xi.

Cretæ Preparatæ, parts, iv. M.

S. Three to five grains every two, three or four hours. Each powder should be thoroughly mixed with a teaspoonful of sugar, a few drops of water being added until the powder and sugar are fully moistened, and then water added to render sufficiently thin to be easily swallowed. This precaution is needful, as the dry or imperfectly mixed powder is acrid.

A formal prescription may be used to represent a five grain dose:

R. Acid, Salicylic, gr., xxx.

Cretæ Preparatæ, gr., x.

Syrupi, ℥ij.

Aquæ, q. s., ad ℥ij. M. Shake well.

S. Two teaspoonfuls every two, three or four hours. The frequency of the dose to be regulated by the frequency of the movements.

—ABLATION OF THE UTERUS was performed "by mistake" by a mid-wife in Germany. In a case of labor attended by her, she tore away, with violence almost inconceivable, what she considered "the rest of the after-birth," but which was in reality the whole of the uterus. The ovaries and fallopian tubes remained in abdomen. The uterus is preserved at Halle. The woman did not die, but on the contrary made a rapid recovery, and has since enjoyed good health.

—HYDRATE OF CHLORAL.—Dr. H. H. Kane, of New York City, specially requests members of the profession with any experience whatever in the use of the Hydrate of Chloral to answer the following questions, and give any information they may possess with reference to the literature of the subject:

1. What is your usual commencing dose?
2. What is the largest amount you have administered at one dose, and the largest amount in twenty-four hours?
3. In what diseases have you used it (by the mouth, rectum or hypodermatically), and with what results?
4. Have you known it to affect the sight?
5. Have you ever seen cutaneous eruptions produced by it?
6. Have you known its temporary or continued use to affect the sexual organs? If so, how?
7. Do you know of any instances where death resulted from or was attributed to its use? If so, please give full particulars as to disease for which given; condition of pulse, pupils, respiration and *temperature*; manner of death; condition of heart, lungs and kidneys; general condition, age, temperament, employment, etc., etc., etc. If an autopsy was held, please state the condition there found.
8. Have you seen any peculiar manifestations from chloral—as tetanus, convulsions or delirium?
9. Do you know of any cases of the chloral habit? If so, please state the amount used, the disease for which the drug was originally administered, the person's temperament and the present condition of the patient. Give full particulars of effect on each organ, system or apparatus.
10. Have you known the use of chloral to predispose to, or directly cause, insanity?

Physicians are earnestly requested to answer the above questions, in order that the resulting statistics may be as full and valuable as possible. Stamps will, in every instance, be returned to correspondents.

All communications will be considered strictly confidential, the writer's name not being used when a request to that effect is made. Address all letters to Dr. H. H. Kane, 191 West 10th Street, New York City.

—ABSCESS OF LIVER.—In the *Va. Med. Trans.* is a paper by Dr. Marion Sims on Abscess of the Liver, giving his own and Dr. W. A. Hammond's experience in its cure by aspiration. One of the patients thus cured was Dr. Gaillard, formerly editor and professor in Louisville; eight and a half ounces of pus were drawn off, and Dr. G. is now in good health, although he had suffered so much and so long that he well nigh despaired of relief. The great diagnostic feature, according to Sims, is cerebral hyperæmia, producing inveterate insomnia. If your patients cannot sleep, palpate the liver—especially if they have had a malarial history. Dr. Hammond claims to have aspirated the liver for abscess twenty-six times, and to have struck pus in fifteen of these cases;

cures were effected in the latter. In the eleven other cases no harm was done.

—SEA SICKNESS.—A Practical Treatise. Its Symptoms, Nature and Treatment. By George M. Beard, A.M., M.D., New York: E. B. Treat, 1880. This little book of 72 pages appears in time to accommodate the great exodus of 1880. The bromides are advised as the best known remedy and prophylaxis, given in large doses. Bromization should be established before reaching rough water, and afterwards kept up, if necessary.

—THE POPULAR SCIENCE MONTHLY for August has a variety of contributions entertaining to physicians. Among these we note Dr. William See's brief argument that the premature burial of persons not dead, but in a trance, is extremely rare. *En passant*, he refers to the account given in Vol. III. of our PROCEEDINGS, of the case of the late Dr. Groux. He claims that the *ordinary* physician is entirely competent to recognize the distinction between a state of trance and that of death. He recommends to non-medical observers of unattended cases where trance is suspected to place a piece of looking-glass, or a dish filled with water or mercury, upon the chest of the person lying supine, and allowing the light to be reflected upon the surface; the slightest respiratory or cardiac action will result in oscillation.

—FOR THE INFORMATION of the medical men in the city, the Society ordered, at its last meeting, the publication of the new Registration Act in the present number of THE PROCEEDINGS. The County Clerk will have the Registration Book ready about August 15th. The affidavits may be made before any person qualified to administer oaths, and must be presented at the time of registration. The severe penalties attaching to the non-attendance to this matter before October 1st make it incumbent on all to give it early attention.

—THE CONSULTATION ACT, to which attention was called in the April number of THE PROCEEDINGS, did not pass the Legislature.

—“HA, how are you? Where have you been this month past?”

“I've been laid up, sick—not been outside of my bedroom door for more'n three weeks.”

“Dear me; and what was the matter with you? Something serious, it must have been.”

“Oh, no, it was a very little thing, but then I was treated by a very big doctor.”

ODD NUMBERS OF JOURNALS NEEDED TO COMPLETE FILES.

THE LIBRARIAN would be very glad to receive copies of the following journals, or would be pleased to negotiate for the same. A great favor to the Library would be conferred:

AMERICAN JOURNAL MEDICAL SCIENCES.

1827 to 1844, inclusive.	1847, Jan., April.
1845, April, July, Oct.	1848 to 1855, inclusive.
1846, All.	1858, Jan., July, Oct.

PSYCHOLOGICAL JOURNAL.

Volume V. and No. 1, Vol. VI.

PRACTITIONER (LONDON).

Volumes I., II. and III.

LONDON LANCET.


1823 to 1848.	
1848 Jan., Feb. and July, to Dec., inclusive.	1849, All.
1850.—January to June, inclusive.	

RANKING'S ABSTRACT OF THE MEDICAL SCIENCES.

Vol. 2, No. 1.—January to June, 1846.	Vol. 39.—January to June, 1864.
No. 16.—July to December, 1852.	Vol. 40.—July to December, 1864.
Vol. 36.—July to December, 1862.	Vol. 41.—January to June, 1865.
Vol. 37.—January to June, 1863.	Vol. 42.—July to December, 1865.
Vol. 38.—July to December, 1863.	Vol. 43.—January to June, 1866.

MEDICAL AND SURGICAL REPORTER, (PHIL.)

1 to 104, inclusive, 106, 107, 109, 110, 112, 113, 114, 115, 116, 117, 118, 119, 120, 121, 122, 123, 125, 126, 159, 162, 163, 164, 168, 169, 170, 179, 180, 181, 182, 183, 186, 190, 192, 193, 194, 195, 196, 197, 198, 199, 201, 202, 203, 205, 206,

 Revised to August 1, 1880.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The August meeting will be held on the 17th, at which there will be presented the following papers:

“Calcium Salicylate in the Serous Diarrheas of Infants,” by Dr. A. Hutchins.

“Antiseptic Excision of the Knee-joint, with Table of Sixty-four Cases,” by Dr. G. R. Fowler.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President</i>	C. JEWETT, M.D., 307 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
<i>Secretary</i>	R. M. WYCKOFF, M.D., 532 Clinton Ave.
<i>Assistant-Secretary</i>	J. H. HUNT, M.D., 419 Hart St.
<i>Treasurer</i>	J. R. VANDERVEER, M.D., 301 Carlton Ave.
<i>Librarian</i>	A. HUTCHINS, M.D., 796 De Kalb Ave.

CENSORS.

B. A. Segur, M.D., 281 Henry St.	F. W. Rockwell, M.D., 6 Lafayette Ave.
E. R. Squibb, M.D., 36 Doughty St.	L. S. Pilcher, M.D., 4 Monroe St.
J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK.

(1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI., Art. 2, of By-laws: “Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate.”

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Dr. R. M. Buell
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

SEPTEMBER, 1880.

No. 7.

CALCIUM SALICYLATE IN THE SEROUS DIARRHEAS OF INFANTS.

BY ALEXANDER HUTCHINS, M.D.

The fact that the writer has treated in private practice, within the past three months, some twenty-seven cases of serous diarrhea in infants, ranging from two months to two and one-half years of age, using one drug only; that some of the cases were seen but once, many only twice, and none above four times; that, in all, the disease is known to have been promptly and permanently controlled, justifies putting this memorandum on record, that others may have the opportunity of testing its efficiency and studying the limits within which its usefulness may be relied upon.

In the last volume of the St. Thomas' Hospital Reports, appeared an article on the "Salicylates of Calcium and Bismuth in the Diarrheas of Infants," giving the theoretical reasons for their use and reports of cases, wherein occurred the statement that the treatment had been found satisfactory in the dispensary practice connected with that institution.

That it had been found useful in dispensary practice was an efficient recommendation. I called Dr. Armor's attention to the fact that I proposed using it when the opportunity presented. Within a short time he reported that he had applied it in one case, where it had responded well and promptly.

My first experience occurred a few weeks later, and as the case was so pronounced and typical, a brief narration thereof will preclude the necessity of further clinical details.

The 25th, 26th and 27th of May were three excessively hot days that ushered in the summer.

On the 25th, a child two years of age, in perfect health and in good surroundings, had been playing most of the day in the open air, exposed to the sudden onset of the intense heat. After a somewhat restless night, at 4 o'clock on the morning of the 26th, the child had a copious movement of the bowels, thin and discolored. Vomiting soon after occurred. Following speedily were other dejections, rapidly assuming the watery character. Accompanying these were frequent vomitings of a thin, watery consistence; after two hours the dejections became more frequent, varying from 3 to 10 minutes apart, discharged without effort, sometimes small, then again profuse, always colorless. The vomiting occurred after each ingestion of food, water or ice, and frequently independent of these, the rejected material being like the dejecta, watery and colorless; a rapid prostration ensued, the patient soon offering no resistance to the frequent change of the napkins, and indifferent to the vomiting. For about twelve hours the temperature remained about 105° and the circulation about 140.

The case was in no respect an unusual one. The city is full of such experiences in the summer season. They are always exasperating, frequently and rapidly fatal. The difficulty in treating them is well known. The tendency to collapse is always imminent.

I commenced the calcium salicylate at seven in the morning, giving one and two grain doses every hour, and followed the treatment patiently till two in the afternoon, without making any impression on the frequency or character of the vomiting or dejecta. The condition was serious. It was my first experience in the use of the drug. I was timid as to deserting the old lines of treatment. However, at 2 P. M. I gave it in five grain doses, and the effect was, as near as may be, immediate. Within a half hour began a recognized control of the movements, a cessation of the vomiting and a lowering of the temperature, accompanied by a softening and moisture of the surface. The medicine was repeated every two hours till 10 P. M., when the disease was under control. But three movements occurred between that hour and morning, and on the following day a natural movement was voided.

The foregoing experience was repeated many times during the past two and a half months in cases closely allied to the one related. The indications of treatment seemed to be pretty clearly defined. Whenever the dejecta were of the serous character, whether the flux was more or less profuse,

in all the cases where the tendency is to cholera infantum, when collapse is to be looked for from excessive drainage of the serum, the calcium salt acted promptly in checking the frequency of the movements—ultimately in controlling them.

The cases on which this memorandum is based are selected so far as to include all those with the more or less profuse watery alvine evacuations, with or without vomiting, and to exclude all others. The purport of this memorandum is to put on record the fact that these discharges were controlled by the calcium salicylate with a promptness and efficiency that the writer has never experienced by any other mode of treatment. The patients ranged in age from two months to two and a half years. No discrimination was made as to diet, which, in some instances, was breast milk exclusively, in others, condensed milk, the patent foods or a mixed diet. In no case was any modification of the previous diet called for, save in the matter of quantity. All the patients were in good social and hygienic surroundings. In two instances the infants were at their summer homes, and the telegraph and mail related the symptoms and conveyed the medicine. In all cases the dose was 3 to 5 grains from 2 to 4 hours. The total quantity consumed by each patient varied between 6 and 18 powders. In a few cases minute doses of aconite and veratrum were given during the stay of the high temperature, and in other few, small doses of quinine were followed up after the subsidence of the disease.

One short series of observations on one drug, in a limited number of cases, in the midst of one set of social influence, during a part of one season, establishes no principle, enforces no rule of action, settles nothing absolutely; but when this series has shown an unvarying issue, it would be more ungenerous to withhold the experience than deficient in modesty in not waiting for further facts.

It was noted that the medicine seemed to have no influence in changing the secretions so as to modify the character of the evacuations. The discharges would be under control for a time, say from 2 to 12 hours, and the next movement would be a watery one, but there would be no further recurrence of the diarrhea. There might be a return to normal movements, or there might be a change to a diarrhea of indigestion, or to a diarrhea from irritation of the mucous surface, each of which would require some special interference. These sequelæ were exceptional, but in no case did the serous discharge recur.

It was noted, likewise, that this treatment necessitated very little interference with the usual diet of the child. It would be nearer the exact fact to say that no interference was required. In the majority of cases the discharges were so promptly checked that an indigestion did not occur.

It was further noted that the calcium salt had no appreciable effect on any one of the other forms of intestinal flux, whether lienteric or inflammatory. The serous diarrhea alone seemed to be amenable to this drug. Each of the other forms required special treatment.

An additional fact was noted, that the vomiting accompanying these diarrheas was controlled so soon as the medicine began to show its effect on the discharges. Certainly without exception the stomach tolerated the presence of the drug.

Appended are some of the observations of Mr. Walter Kilner (M. B. Cantab.), a portion of which I have been able to verify:

“One of the most successful set of cases was when the diarrhea seemed to be entirely dependent upon the heat of the weather, or, during the autumn, upon the change from the cold nights to the hot days—at least when no other cause for it could be discovered. It seems not improbable in these cases that the diarrhea is an effort of nature to reduce the temperature of the body, the sweat glands being either incompetent or else not sufficiently active to perform the extra duty suddenly required of them. Here the salicylates not merely check the diarrhea but also cool the body by their influence upon the sweat glands, assisted by the direct cooling action of the air upon the larger quantities of blood impelled through the cutaneous capillaries, and, in addition, the stimulation of the glands does not terminate with the leaving off of the medicine, thus enabling the child to become accustomed to the hot weather.

“Another class of cases in which these drugs are useful, is when the diarrhea occurs either subsequent to, or contemporaneous with, the gastric catarrh of infants, whether arising from improper food or from dentition; also a certain amount of benefit may be expected when the stools are foetid. In all these instances there are most likely decomposing or fermenting substances in the alimentary canal, accompanied by flatulence and discomfort, if not pain. By the arrest of the decomposition the local irritation caused by the offending material is removed, which may assist in controlling the peristaltic action of, and the secretion into, the bowels; or even should the flux remain, the condition of the patient is improved.

“Whenever the diarrhea is due to dentition, with the single exception mentioned above, we can hardly expect, and we rarely, if ever, find any good to follow the use of these salicylates, because the derangement of the alimentary canal is only secondary—being derived by reflex action from the erupting tooth, and they do not exert any influence upon the fifth nerve.

“Another contra-indication of the use of these salicylates is when the food is passed through the alimentary canal so quickly as to be quite or nearly undigested. To obtain full benefit from these salts, they must be given in good-sized doses; for, although the smaller ones will remove the fœtor, yet the number and quantity of the evacuations will not be diminished by them. During their exhibitions for diarrhea they give rise to several effects worthy of notice. First, as long as the diarrhea lasts the skin continues dry, or, at the most, regains its natural moisture; but this moisture varies inversely to the purging; nevertheless, even when the stools have become solid, there is rarely, if ever, much perspiration, although the same sized doses, when given for other illnesses, cause the child to be bathed with it.

“The temperature of the patient, when taken in the axilla, has a tendency to come to the natural standard, falling when higher, and rising when lower; yet during convalescence the temperature is usually below the normal. This regulation of tempera-

ture may be accounted for by the cooling powers of the drugs when the body is too hot ; and, on the contrary, which is much more common, when the temperature is subnormal, the rise is due to the increase of blood to the surface, as it is most likely that, had the temperature been taken per rectum it would have been found above the natural point.

“The pulse, as far as I was able to judge, is unaffected directly by these drugs; any alteration of it seems to be dependent upon the state of the child. The tongue, if clean, remains so, or if furred, becomes clean; and should it be dry, moisture returns at the same time as the skin loses its pungent feeling. Only one more fact remains to be noticed, namely, the effect of these drugs upon the brain. I have never seen a case of diarrhea in which the usual head symptoms of salicylic acid have shown themselves, although with the same dose during other illnesses they have appeared. For this peculiarity I can offer no explanation. The best doses of these drugs are two or three grains for a child under six months, and from three to five, or more, when above that age. The bismuth salt is preferable when vomiting is present, otherwise the calcium salt will be equally beneficial.

I am well aware that, as a purism in pathologic speech, the term *catarrhal* diarrhea is to be preferred to *serous*, and that the *catarrhal* is the accepted form. It has suited my purpose best to use the term *serous*, to emphasize the symptom, or the resultant of a condition, which it has been my observation to see was favorably affected by the calcium salt.

The excessive watery discharge, the hyper-secretion from the intestinal mucous membrane, is the obvious waste which it is desirable to control. The tendency is to collapse from exhaustion. This tendency is averted so soon as the discharges are stopped or modified. All efforts at support or tone are futile till the flux is arrested. It is therefore of supreme importance to arrest the discharge promptly.

Following on after the experience of Mr. Kilner, my own observations in a limited number of cases, occurring during a portion of one season, have shown that the calcium salicylate has a positive usefulness in the serous diarrhea of infants, and judging from my own observations, its usefulness is limited to the cases characterized by profuse watery flux. In the other forms of catarrhal diarrhea in the congestive and inflammatory forms, where there is organic change in the mucous membrane, and where the discharges are lenteric, there is no evidence of the special usefulness of the drug.

Desirous of including in this report as much experience as was practicable, and especially desirous of having the effect of this medicine observed among children whose hygienic and social surroundings were open to criticism, I appealed to DR. JEROME WALKER, whose experience would give weight to any observations he might make, to test this drug at the Sea-Side Home for Sick Children at Coney Island, of which he is Medical Superintendent. The following letter, received from him, is appended in full.

8 Seventh Avenue, Brooklyn, August 14th, 1880.

MY DEAR DOCTOR : Agreeable to your desire, I have used the Salicylate of Calcium Mixture at the Sea-Side Home in the diarrheas of children, with the results as shown in the annexed reports of cases taken from my note-book.

I have given the medicine in doses of 3 grs. every two hours, mixed thoroughly with simple syrup. Thus prepared, the medicine is more easily taken than when mixed with sugar and then moistened. Maple syrup might be better—or even syrup of Gum Acacia. Generally from five to eight doses were necessary before any good result was perceptible. The earlier doses were often vomited in whole or in part. Of course my experience with the medicine has been too limited to enable me to state *exactly* in what cases it should be used to the exclusion of pepsin, bismuth, rhubarb and soda, etc. But the following records of a few cases out of thirty recorded may assist *you*, though the meteorological conditions are, unfortunately, not stated :

CASES.

July 26, 12 M.—F. C., two years. Always sickly. Last two weeks several green watery movements a day. No vomiting. Fed from table. Is delicate. R., Mist. Sal. Cal.

27, 12 M.—No movement in night, two to-day. Light yellow. Thicker.

28, 7 A. M.—Three movements in night. Yellow, but much *thinner*. (No cause but damp night.)

30, 7 A. M.—Cried all night, but bowels all right. Went home on 31st quiet and well.

July 26, 7 P. M.—N. R., one year. Nursed and fed always. Sick for forty-eight hours. Diarrhea. Five movements per day. Greenish. R., Mist. Sal. Cal.

27, 12 M.—Rested well last night. No movement, but three this morning; watery, yellowish, curdled. (Mother fed baby *gruel*.) R., Con. medicine.

31, 12 M.—Went home all right. No movement in night and two to-day; natural. Mother states that “after the ninth dose child began to mend.”

July 26, 12 M.—K. McC., 14 months. Is being weaned. Sick three weeks. Vomiting and diarrhea. (Six to twelve movements a day; thin, gray.) No vomiting to-day. R., Mist. Sal. Cal.

31, 12 M.—After three doses, child better. To-day two movements, natural, and but one in night.

July 26, 12 M.—F. C., two years. Sick one month. Diarrhea and vomiting. For one week prolapsus ani. No blood passed. Many watery movements per day. To-day none. R., Mist. Sal. Cal.

7 P. M.—Passed no water to-day. Feverish. R., Aconite and Spts. Nitro and inunction.

28, 12 M.—No fever. Vomited after overfeeding. Movements natural as long as Salicylate Mixture given. Gave out, and bismuth mist. did not answer as well.

August 2, 12 M.—T. R., 16 months. Nurses—wasted. Sick three weeks, with no vomiting, ten movements a day; watery. R., Pepsin.

3, 12 M.—Hasn't slept well. No movement in night, but twice to-day; green still. R., Sal. Cal. and Bromide Mixture.

5, 7 A. M.—Better. Sleeps well. No movement since 12 M. yesterday. R., Brandy. Con. Med.

6, 7 A. M.—Well. Movement natural.

This child had a relapse at home from improper feeding. Returned and improved on the Sal. Cal. Mist.

August 2, 5 P. M.—H. O., nine years. Sick two days. Sudden watery diarrhea, with pain. Ten movements per day. R., Sal. Cal. Six doses relieved entirely.

Of course the diet is to be closely watched when this medicine is given, as with all remedies for diarrhea, though I hope yet to test the medicine without giving special directions as to food. In my experience, so far, the medicine is especially adapted to serous diarrheas from whatever cause. Can often be combined to advantage, in excitable, fretful children, with Goodell's Bromide Mixture; is *not* good in cases of dysentery; rather seems to aggravate the disorder; is a valuable addition to our stock of remedies.

Thanking you for calling my attention to the mixture, I shall continue to use it, and hope at some future day to be able to compare its usefulness with other well-tried remedies.

Respectfully yours,

JEROME WALKER.

Assuming the correctness of these observations, the time has not arrived for any positive statements of the *modus operandi*. The causes that operate to produce catarrhal diarrhea in infants are numerous, and to no one of the forms of this catarrhal diarrhea is there any characteristic pathologic condition. The common consent of post-mortem states, as well as the argument from the disease during life, and when it does not go to a fatal issue, is that this serous diarrhea results from a simple functional lesion, and that this lesion is a hyperæmia over a greater or less extent of the intestinal tract. This hyperæmia is accompanied by an elevation of temperature local to the part affected, and the excessive peristaltic action is a contingent on the excess of fluid poured into the intestinal canal.

The literature of this general subject is extensive enough to satisfy the most omnivorous reader; but Dr. Wyckoff, of this Society, in a paper on "Mortality and Temperature" (See PROCEEDINGS, November, 1878) has pointed out much that is pertinent to the etiology of this disease.

Concurrent with this is the undefined status of salicylic acid in therapeutics. At the least the acid is recognized as an apyretic, a depressor of animal heat, under certain conditions, and that it arrests fermentation. Mr. Kilner alleges that he was led to use it in infantile diarrheas from having observed its effect in producing diaphoresis in a case of variola, and arguing that, if it derived to the skin, it would relieve the pressure on the intestinal tract. I have been totally unable to verify his statements as to its derivative action on the skin, and am more inclined to hold that, in its power of reducing the abnormal heat of the intestines, in arresting the irritant action of the products of the organisms, which are the active agents of putrefaction and fermentation, either or both, is to be traced the efficiency of the calcium salt in these infantile diarrheas.

There has been no inconsiderable use of salicylic acid in various intestinal discharges. It is proper to again refer to our own observers. Dr. Mat-

tison presented to this Society (See PROCEEDINGS, January, 1877) a plea for salicin in chronic diarrhea. The present writer failed to verify Dr. M.'s experience, after faithful trial in the Brooklyn City Hospital, probably failing to discriminate the cases to which salicin is applicable.

Dr. Segur, of this Society, found the sodium salicylate useful in controlling the chronic diarrhea of adults. The present writer called attention to this in the PROCEEDINGS for Feb., 1880. The sodium salt cannot be pushed with the same freedom as the calcium salt, as its effect upon the brain is too prompt and too severe.

The writer has made a few experiments with minute doses of salicylic acid in mild cases of serous diarrhea, and with sufficient promise to justify further investigation.

In every case, thus far, the writer has dispensed the medicine himself in the sick room or in the office. He had the knowledge of using a properly prepared drug, and the advantage of immediate service. Besides, he was scrupulously careful to show the attendant how best to administer the medicine. By mixing the 3 or 5 grain dose in a teaspoonful of sugar, adding a few drops of water at a time till the whole is thoroughly moistened, and then thinning it with water so that it can be easily swallowed, is a sufficient device, though demanding a little patience.

I append Mr. Kilner's chemical and pharmacal notes:

"Before concluding it may be well to say a few words about the chemical composition of these salicylates. The calcium salt has a formula of $\text{Ca} (\text{C}_7 \text{H}_5 \text{O}_3)_2 \text{H}_2 \text{O}$, crystallizes in octahedra, and is very soluble in water. The bismuth compound is $\text{Bi}_2 (\text{C}_7 \text{H}_5 \text{O}_3)_6 \times \text{H}_2 \text{O}$, or $\text{Bi} (\text{C}_7 \text{H}_5 \text{O}_3)_3 \times \text{H}_2 \text{O}$, and is amorphous, at least when prepared by digesting the teroxide of bismuth with salicylic acid.

"These preparations are nearly tasteless, and slightly sweet. The manner in which the drugs were prescribed was to mix the acid with either the prepared chalk or with the oxide of bismuth in the proper proportions, namely, 276 parts by weight of the acid with 100 of chalk, which will form 314 parts of the anhydrous salt, or, what is sufficiently correct, 11 of the acid to 4 of chalk, making about $12\frac{1}{2}$ of the salicylate of calcium. For the bismuth salt 828 parts of the acid is required for 468 parts of the bismuth oxide, forming 1,242 parts of the anhydrous salt; or, simplified, about 12 of the acid to 7 of the oxide, forming about 18 of the salt."

The following prescriptions contain five grain doses of the salicylates.

R.—Acid salicylic, gr. xxx.

Cretæ precip., gr. x.

Syrupi, ℥ii.

Aquæ, ℥xiv. M.

Two teaspoonfuls every 2 to 4 hours.

R.—Acid salicylic, gr. xxvi.
 Bismuth teroxid, gr. xiv.
 Tr. hyoscyami, \mathfrak{J} i.
 Syrupi, \mathfrak{J} ii.
 Aquæ, \mathfrak{J} xiii. M.

Two teaspoonfuls every 2 to 4 hours.

The form in which I have used the calcium salt would be represented in a formal prescription thus:

R.—Acid salicylic, gr. xxii.
 Cretæ præparat. gr. viii.
 Misce accurate.

Divide in chart. No. vi. (gr. v.), vel. No. x. (gr. iii.)

Sig. one every 2 to 4 hours.

I found the calcium salt so effective that I abandoned the bismuth salt mainly to avoid the discoloration of the discharges due to the bismuth. I did not find that the bismuth acted any more effectually than the calcium in controlling the vomiting.

Two more facts of interest remain to be stated. In the process of mixing the powder an effervescence occurs, which alarms the attendant, but which the prescriber recognizes as due to the release of carbonic acid in the formation of the new salt. Also in the process of mixing, a pungent odor of chlorine is not infrequently perceived. Dr. Squibb informs the writer that this is probably due to the impurity of the prepared chalk. The prepared chalk of the shops is a residuum of the manufacture of chlorinated soda, and if the chalk be imperfectly washed an odor of chlorine will be perceived. This is an impurity of the drug and should be avoided.

This memorandum is submitted with what seemed to be a few needful comments. If this treatment avails in the recurring seasons, in this special form of diarrhea, Mr. Kilner may well be congratulated. If it fails, or if it be but occasionally and accidentally useful, it will be but one more disappointment to be stuffed into the bursting rag-bag of the ages.

DISCUSSION.

DR. CHASE was very glad to know that the modesty of the Doctor had not deterred him from presenting this paper at this time. Personally, he felt under obligation, for if there is a drug of which we have no knowledge which has a value in controlling the serous diarrhea of infants, it is well that this Society should know it. Probably the season is not so far advanced as yet but that we shall have an opportunity to try this drug. Certainly the mortality of children this summer is of such a character that if there is any drug which even promises what this seems to promise, there is every reason why we should test it thoroughly, and compare our experience with that of the Doctor, to know whether it will accomplish what we have every reason to expect it will.

DR. READ said there were one or two questions he would like to have the Doctor answer, viz.: with reference to the diet in the cases cited by him, and as to whether external applications were used, or stimulants applied.

DR. HUTCHINS replied that he took especial pains to say that the cases were treated by the drug only, and that in no case was there any other means or device resorted to for the relief of the patient.

DR. READ said that he was very glad to hear this. He said he must confess himself somewhat skeptical in regard to the use of new remedies for the immediate and thorough checking of summer diarrheas, and especially as to the salicine treatment. He had made use of that remedy himself, and was much disappointed in it. Dr. Mattison had requested him to try it, and he had given it a very thorough trial in 1877; and though answering in some cases it was not as efficient as many other remedies. He had not used this remedy of Dr. Hutchins, but should assuredly do so the rest of the season. He thought the fact that 27 cases, many of them serious ones, had been cured in that way, by the administration of a single drug, without the application of mustard and without a change of air, was something remarkable.

DR. SEGUR said that if it would be in order, he would like a few words from Dr. Hutchins as to his method of treatment in a similar class of cases last year and the comparative promptness of the results.

DR. HUTCHINS stated in reply that if he were to do that he would have to go over the whole history of the therapeutics in those cases. The treatment has always been thoroughly unsatisfactory. He recalled with great distinctness a notice which appeared, about the year 1870, in the *Philadelphia Medical and Surgical Reporter*, where a gentleman the previous year had had such marvelous results in cholera infantum cases, and with so simple a prescription as: Bromide of potassium 10 grains, orange flower, 1 ounce; 10 drops every four hours. During that summer that medicine seemed to act like magic in every case. The next year he commenced with flying colors, and in every case he had a blank failure. As to the cases where the salicylate had been used, the treatment all through had been with this one drug, with very little alteration of diet, and with a remarkable action upon the serous discharges. That was the point he had made all along. He wished to say further that he disclaimed all originality in this matter. He based his treatment entirely upon the results obtained by Mr. Kilner.

DR. READ said that he was very much obliged to the Doctor, as he knew all the Society were, for giving them new light on the subject. With regard to the efficacy of certain remedies at certain times, he would mention another remedy, given to him by his friend Dr. Dodge some years ago, with which the latter said he had had marvelous success. This was the fluid extract of colocynth. (Fl. ex. colocynth (prepared from solid ex.) *mi.* in ℥iv . water. *S. mv.* of mixture *p. r. n*.) He tried that, and he thought he had discovered a remedy for the diarrheas of children. It acted in the way that Dr. Hutchins described, and acted marvelously. His subsequent experience was like Dr. Hutchins'. Since that time he has had no luck with it at all. There had occurred at various times in his practice a series of cases, which he had treated sometimes with one remedy and sometimes with another. One remedy might do well one year in a certain locality, and perhaps the next year it would fail in that very locality. In his opinion, there must be local causes affecting these cases of serous diarrheas as they do diseases of any kind, both of children and adults. In a certain locality in Columbia Street, a very crowded and unhealthy part of the city, the children commenced to do badly during the hot spell in May, and they have done badly ever since, at all times and under all varieties of treatment; and under his old standard of treat-

ment, which he had adhered to many years, in another part of the city, not very far from that, in tenements equally crowded and equally filthy, there had hardly been a death in the whole number of cases; the two portions of the city furnishing about the same number of cases.

DR. KRETZSCHMAR inquired whether Dr. Hutchins obtained the drug at the drug stores or prepared it extemporaneously.

DR. HUTCHINS answered that the proper title of his paper should have been "Ex-temporized Calcium Salicylate"—that it was made extemporaneously.

DR. GILFILLAN expressed himself sorry that he had arrived too late to hear the paper read. He had been largely interested in the subject for some years past. He felt, as many other gentlemen in practice felt, that we are undergoing a change at the present time as regards the treatment of this class of cases. If we look at the records of cholera infantum and summer complaint, and diseases classified in that way, we will find that a large number of these diseases have a variety of treatment suitable to special and individual cases. Very frequently, in the summer time, children who are not nursed by the mother suffer from inanition, which is a cause of cholera infantum. Food is given to the child in a condition not proper for nourishment; it is either in fermentation or ready to undergo fermentation, and is not fit to digest. The child takes some of that nourishment, and, in half an hour, it will vomit it up. The point was that cholera infantum is a disease consequent upon a high temperature, and that is owing to a deteriorated condition of the nervous system. We want to give something that will stimulate the nervous system, and, if there is any trouble from dentition, we should give something that will relieve that. Perhaps salicine is more likely to arrest fermentation than anything that we use in ordinary practice. He had used it largely for some years past in forms of diarrhea, and also for the last two years in forms of cholera infantum and some other diseases in children, and had been perfectly satisfied with the result. Salicine itself, he thought, was one of the most valuable remedies to check diarrhea. It prevents fermentation, and prevents that peculiarly putrescent odor of cholera infantum. Pure water, bathing, and attention to the food constitute the rational and scientific treatment for cholera infantum.

DR. SULLIVAN stated that he had had a little experience with salicylate of calcium. The first case where he had used it was that of a lady who had diarrhea all last summer, and this summer when the warm weather set in it began again. He gave her a prescription according to the formula. It acted like a charm. She said she had never had anything to act like it. The next case was a little child. It commenced with a cholera infantum, and ran along until it became a quite serious diarrhea. He treated the child with salicine and bismuth, and it did very well. A few days afterwards he called again. The child had had a relapse. He prescribed salicylate of calcium. He called the next day and the child was very much worse, and the mother insisted that the medicine made the child worse. It had passages every fifteen minutes. He could not account for it, but since that time he has been afraid to prescribe it again. His chief point was, whether in the case of the child there was an inflammation—an inflammatory diarrhea. He was inclined to think there was. He said he would like to inquire of Dr. Hutchins if he thought the salicylate of calcium answers well where there is inflammation.

DR. HUTCHINS said that, with his brief experience, he could not answer the question *ex cathedra*. Dr. Walker, in his communication, made a statement which he (Dr. Hutchins) had had some opportunity of confirming—that in dysenteric cases the salicylate aggravates the trouble.



FIG. 1. From cast of limb made prior to operation.

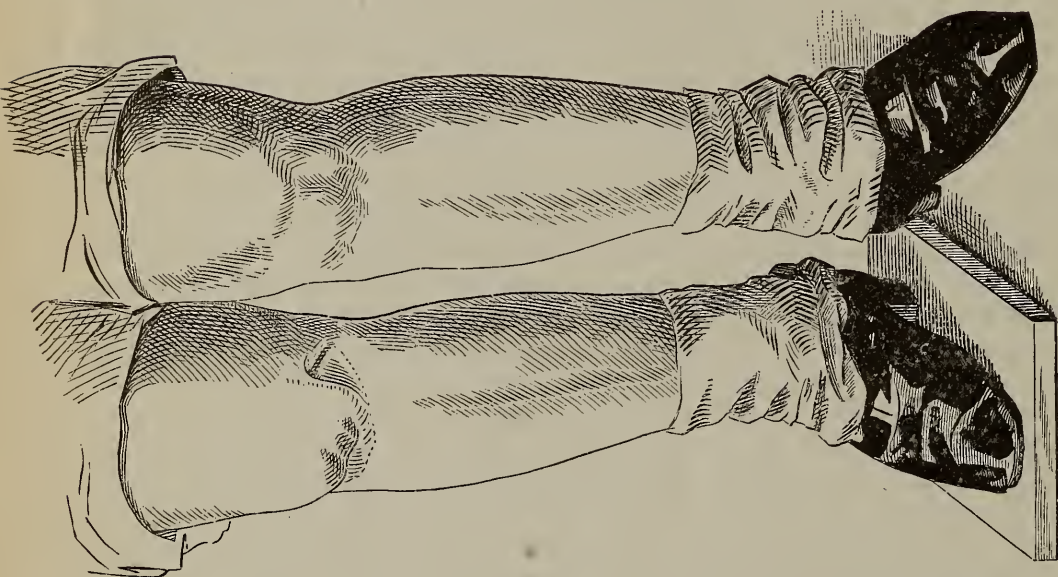


FIG. 2. From life, 20 months after operation.

DR. GEO. R. FOWLER'S CASE OF EXCISION OF THE KNEE-JOINT.

ANTISEPTIC EXCISION OF THE KNEE-JOINT.

BY G. R. FOWLER, M.D.

Case: Mrs. S., widow, æt. 40, German. This patient came under my care in August, 1878, suffering from chronic arthritis of the knee-joint of one year's standing. She suffered a slight injury just prior to the development of the trouble, but it was of so slight a character that but slight attention was paid to it at the time. The plaster cast (Fig. 1) exhibits the condition of her limb as it appeared at that time. There existed a subluxation of the tibia and fibula backwards, the crucial ligaments being destroyed and the powerful flexor muscles forcing the upper extremities of the tibia and fibula into the popliteal space. She suffered the usual symptoms of chronic articular inflammation, nocturnal pain, etc. On November 15th I made the attempt under ether to reduce the luxation and retain the proper relations of the structures comprising the joint by means of a plaster-of-Paris dressing. The attempt was a failure, however, and after a protracted trial of the extension and counter-extension method with the limb elevated, I finally made the proposition, which was accepted, to excise the joint.

On December 18th, 1878, I proceeded to perform excision of the knee-joint under the carbolized spray of Lister, assisted by Drs. Lewis S. Pilcher and J. S. King. Esmarch's bandage being applied the curvilinear incision of Mackenzie was made. After removing the patella the leg was forcibly flexed, the ligaments divided and the soft parts cleared from the bones. A small Butcher's saw was then applied and a slice of the lower extremity of the femur removed by sawing from before backwards, a folded napkin protecting the contents of the popliteal space. The head of the tibia and fibula were then removed in the same manner. A few small vessels were secured by cat-gut ligatures, and a drainage tube of perforated rubber tubing placed in the bottom of the wound, its ends emerging from each upper angle. The sawn surfaces of the bones were then brought into accurate opposition and then secured by silver wire passed through holes drilled in the bones at points corresponding to the attachments of the lateral ligaments. The wires were twisted tightly and the free ends reflected in such a manner as to lie between the sawn surfaces of the bones, where they were allowed to remain. The wound was then closed with horse-hair sutures. A light hoop-iron splint was then secured to the posterior aspect of the limb by a few turns of a roller. Two iron bracket splints were then fastened in a like manner, one to the upper and the other to the outer surface of the limb. A piece of Lister's

protective being applied to the region of the knee, the hoop-iron splints were still further secured in position by a plaster-of-Paris dressing, extending from the base of the toes to the lower margin of the incision, and again from the upper angles of the curvilinear incision to the upper portion of the thigh. This dressing entirely encased the whole limb except the region of the wound, holding securely in position the bracket splints, and these in turn firmly fixing the knee-joint. In the space left vacant the dressings were applied, consisting of the before-mentioned protective and a thick layer of Westhorp's antiseptic manure lint of carbolized jute, the whole being covered by a piece of Mackintosh cloth secured by a flannel roller.

From the moment the patient was removed from the operating table she gave me not a moment's uneasiness. She rallied without any difficulty from the shock, and her convalescence proceeded both rapidly and satisfactorily. The after treatment consisted in changing the dressings about once in twenty-four hours under the Lister spray. At the end of the third week, assisted by Dr. H. C. Rogers, I removed the splints and found that the bones had united in a very satisfactory manner. As a precautionary measure, however, the splints were reapplied in the same manner as at first and allowed to remain undisturbed until the seventh week, when, upon again removing them, firm union was found to have taken place and the patient allowed to walk about. This she was able to do without any support. The silver wires were not removed, the incision having healed by first intention, and no traces of the retained wire were even visible after closing the wound.

Although the bones were placed in line after the operation, and the sawn surfaces accurately fitted together, it is now evident that there is slight genu-varum. (Fig. 2.) This has been observed before by other operators, and it is advised that the limb be placed, after operation, in a position of genu-valgum, in order to overcome the subsequent tendency to bow-leg.

This patient's surroundings were, at the time of the operation, and subsequently during her convalescence, of the worst possible character, from a sanitary point of view; they were well calculated to tax to the utmost the claims made in favor of the Lister plan of antiseptic treatment. She occupied the middle floor of a filthy tenement-house in the most crowded and dirtiest part of the Eastern District. The house is one of two buildings placed upon a lot of 20 x 100 feet; a very narrow courtyard lies between the two houses, and a part of this occupied by an over-filled privy vault at the time. No traps were placed upon the waste-pipes leading from the sinks, and the odor of sewer-gas constantly pervaded the place. The patient's quarters consisted of a small and crowded room in which the patient lay during her convalescence, and in which also the cooking, washing, etc., of the patient's family were done.

The complete immunity which this patient enjoyed, under such unpropitious circumstances, from the dangers and complications incident to excision of the knee-joint by the old method, led me to make inquiry into the subject. Being aware of the enthusiasm of our German brethren over Listerism—an enthusiasm, by the way, not in any degree approached by Lister's own countrymen—I addressed a series of inquiries upon the subject to the best known surgeons of Germany. From the majority of these I received prompt and courteous replies, giving me their experience in antiseptic excision of the knee-joint. The records thus obtained I have tabulated, and herewith present them as a contribution to the literature of antiseptic surgery.

Since the first successful knee-joint excision by Filkins, of Norwich, in 1762, and the almost classical case of Parck, of Liverpool, in 1782, the operation has but slowly gained in favor. During the last twenty years, however, cases have multiplied more rapidly. The tardiness of its adoption may be accounted for by the fact of its great mortality. According to Volkmann, the German surgeons lost upwards of seventy-five per cent. of their cases prior to the introduction of Listerism. This extraordinarily high mortality, however, does not seem to have occurred elsewhere than in Germany; among the English and American surgeons much better results have been obtained. Even with the French surgeons excision of the knee-joint—never with them a popular operation—gave much better results than those stated by Volkmann.

In order to have a substantial basis for comparison between what is called the old method and that known as the antiseptic plan of Lister, I have availed myself of a very exhaustive work by Culbertson,* who has recorded in a tabular form upwards of 600 cases of excision of the knee-joint, operated upon without any especial antiseptic precautions. I have selected from his tables 582 cases of the operation, being all operated upon for disease, in which the results were fully stated. As far as the results have any bearing upon the question under consideration, they may be briefly stated as follows:

Total number of cases.....	582
Deaths from all causes	180
Deaths due to general disease or complications existing prior to operation.....	100
Deaths directly traceable to the operation, such as those due to pyæmia, septicæmia, erysipelas, gangrene, phlebitis, etc.....	80
Recoveries with useful limbs.....	296
Recoveries after subsequent amputation.....	56

* Excision of the Larger Joints of the Extremities. Prize Essay of the American Medical Association, 1876, by H. Culbertson, M.D. Supplement to Vol. 27, Transactions of the American Medical Association.

This is the best possible showing for excision by the methods in vogue prior to the introduction of Listerism.

In addition to the heretofore unpublished cases of excision with Listerism, constituting the table compiled by myself, and hereunto appended, I have to acknowledge my indebtedness to Dr. Nathan Sack, of Dorpat, Russia, for a statistical table placed by him at my disposal.* An examination of the last mentioned gives the following:

Total number of cases	101
Deaths from all causes	21
Deaths due to general disease or complications existing prior to operation.....	13
Deaths directly traceable to operation, such as those due to pyæmia, septicæmia, erysipelas, gangrene, phlebitis, carbolic acid poisoning, etc.....	8
Recoveries with useful limbs	66
Recoveries after subsequent amputation.	6

Turning now to my own table of antiseptic excisions, the following facts present themselves:

Total number of cases.....	66
Deaths from all causes.....	8
Deaths due to general diseases or complications existing prior to operation.....	5
Deaths directly traceable to operation (pyæmia, septicæmia, erysipelas, gangrene, phlebitis, carbolic acid poisoning, etc.).....	3

The great boon claimed to be conferred by Listerism consists, as is well known, in the freedom from injurious irritations caused by the entrance of air laden with septic germs. But the absence of irritation may in its turn have some disadvantages, according to Kocher, Hueter, Volkmann and others. These surgeons ascribe an occasional failure to get firm bony union to the rapid healing of the soft parts, and the lessened inflammatory action in the excision wound. Kocher even goes so far as to recommend that the increased risk be incurred of a higher mortality by treating cases of excision of the knee-joint as open wounds, in order to attain a greater certainty of permanent bony union. He bases this upon the fact that of twenty cases treated by him in the way just stated, in all there occurred firm osseous union; on the other hand, of five cases treated antiseptically, the three recoveries healed with movable joints. Inasmuch as the functional result, now very generally aimed at by surgeons, consists in a firm ankylosis of the parts after excision, this question becomes a very important one. Nor can the occasional rare instances in which an active movement and a useful limb are combined be any justification for lack of care on the part of the surgeon in securing bony

* Beitrag zur Statistik der Kniegelenkresection bei antiseptischer Behandlung. Inaugural Dissertation, von Nathan Sack, Dorpat, 1880.

union wherever possible. One of the rare cases above alluded to took place in the practice of Mr. Annandale in 1872. The patient was a girl aged ten. The operation consisted in a semilunar incision through the integument, removal of the extremity of the femur, leaving the epiphysis intact, and removing a thin slice from the articular extremity of the tibia. The patella was also excised. When the patient left the hospital there was no union, and the case was considered by Annandale as having a very unfavorable termination. After the lapse of five years the patient again came under observation, exhibiting an extraordinarily useful limb, with an active movable joint. As before stated, such a result should never be expected, and will but rarely fall to the lot of any surgeon to encounter. In most instances, unless firm bony union can be obtained, amputation offers the only hope of freedom from the incumbrance of a worse than useless member.

Although the objections urged by Kocher against the antiseptic method in knee-joint excisions on the grounds just stated may be valid, it will require a much larger experience in this class of cases to warrant a surgeon in incurring increased risk of losing his patient by death in attempting to secure by the open treatment a better functional result. Moreover, the statistics as here presented do not bear out Kocher's views upon this point.

An inquiry into the causes rendering so perfectly useless a limb in which there has been a failure to secure a firm union, may not be out of place here. After excision the normal checking or locking apparatus is lost. In the normal condition the checking or locking function is produced by the so-called checking facette of the cartilage of the external condyle of the femur, into which the outer border of the surface of the tibia fits; and it is still further assisted in full extension by the anterior border of the inter-condyloid fossa of the femur resting at the tibial eminence, and by the action of the inner ham-string tendons.

If I were to venture an opinion as to the cause of frequency of non-union in excision of the knee-joint, it would be that it was due to the want of a proper retentive apparatus during the process of healing. In my own case an exceedingly satisfactory result was obtained by the use of permanent silver wire sutures securing together the ends of the bones. In addition to these an immovable dressing was applied above and below the joint, and these connected together by iron bracket splints; not the slightest movement was possible between the sawn surfaces of bone. This method is not new, having been for a long time recognized as the best treatment for ununited fractures. Bidder particularly advocates this practice, conjoining with it or not, as circumstances require, ivory pegs acting as "dowel pins," driven into the extremities of the bones. A case thus

treated antiseptically by Bidder, of knee-joint excision, gave an exceptionally good result in a very short time. Helmer also uses this method of ivory dowel pins and silver wire sutures, and claims that in his hands it never fails to result in prompt and decided bony union.

A very important question has arisen, relating to the arrest of development in the limbs of children who have suffered excision of the knee-joint. Although it may not appear exactly germane to the subject of the advantages of the antiseptic method of operating, yet I cannot refrain from alluding to it. It is now generally admitted as a fact that, in children, a removal of any considerable portion of the lower articular extremity of the femur results in a decided retardation in the growth of the limb. Humphrey confirms this; for, in eighteen cases of children under his observation, the growth was interrupted in all cases in which large portions of the bone were removed. The maintenance of the epiphysis he therefore concludes to be indispensably necessary to the growth of bone in children. Bryck, on the contrary, avers, however, that the retardation of growth in children, after this operation, is not entirely due to the removal of the epiphysis. He was able to point out, in several cases, the fact that, before operation, a faulty growth of the diseased limb existed.

In calling attention to the very great advantages, apparently, of Listerism over other methods of operating, as shown by these statistics, I am not unmindful that the former class of cases is very much smaller, comparatively, than the latter, and that further experience and study may somewhat modify the future aspect of the question. Yet the fact remains undisputed that a very much smaller death-rate is present in those cases in which Listerism was employed, as compared with the old methods.

In the following summary I have presented the most salient points, together with a statement of the comparative percentages of deaths, etc.:

SUMMARY.	Culbertson's table, old method.	Sack's table, antiseptic method.	The writer's table, antiseptic method.
Deaths from all causes.....	30.93 per cent.	20.79 per cent.	12 per cent.
Death due to pre existing disease or complications.....	17 18 “	12.88 “	6 “
Deaths due to those influences supposed to be preventable by the antiseptic method of treatment, including in the writer's table two deaths attributed to carbolic acid intoxication.....	13.07 “	7.9 “	3* “
Recoveries with useful limbs.....	50.86 “	65 34 “	63.6 “
Total number of cases upon which this estimate is based.....	582	101	66

* The remaining 3 per cent. of deaths were due to acute osteo-myelitis and exhaustion.

The following deductions may be drawn from the foregoing study :

1. The total mortality in excisions of the knee-joint has diminished about one-third since the introduction of Listerism into surgical practice.
2. The majority of the fatal cases operated upon antiseptically died of a pre-existing disease or complication.
3. Fatal cases directly referable to the operation and from causes such as are now considered preventable by antiseptic treatment are reduced fully 50 per cent.
4. The functional result is not influenced in a very marked degree by Listerism.

Tabulated Synopsis of Sixty-six Cases of Antiseptic Excision of the Knee-Joint.
By GEO. R. FOWLER, M.D., Brooklyn, N. Y.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
1	Prof. E. Rose, Condon's Hosp., Zurich.	Female, 21 yrs.	Caries of left knee-joint.	Tuberculosis; stupor produced by the use of carbolic acid.			Died 39 hours after operation of collapse, superinduced by carbolic acid poisoning.
2	"	Female, 21 yrs.	Caries of left knee-joint.	Caries recurred; a second operation performed at the end of six weeks.			Amputation performed one year later; amyloid liver diagnosed.
3	"	Female, 9 yrs.	Caries knee-joint.	Caries recurred.			Amputation performed at the end of six months.
4	"	Male, 18 yrs.	Caries of left knee-joint.	Very satisfactory fixation while in hospital; 17 months afterwards known to have bony union.			Discharged from hospital at the end of nine months, wearing plaster Paris splint. Seventeen months after operation, examined and found to be completely cured.
5	"	Male, 6 yrs.	Caries knee-joint.	In collapse after operation; necrosis where ends of bones were united by wire; excessive suppuration.			Final result not stated in report.
6	"	Male, 30 yrs.	Complete and inveterate luxation of the knee-joint.	Stupor induced by carbolic acid.			Died at the end of six hours with symptoms of carbolic acid poisoning.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
7	Prof. F. Ried, Jena.	Male, 12 yrs.	Anchylosis of the right knee-joint.		Bony union in 2 months.	Very slight shortening.	Discharged, cured, at the end of three months.
8	"	Male, 18 yrs.	Caries of the knee-joint.				Amputation of femur subsequently performed, cured. Patient finally died of miliary tuberculosiis.
9	Prof. von Nussbaum, München.	Female, 3 yrs.	Caries.		Bony union in 20 weeks.	3 inches, 1.2 ctm.	
10	"	Male, 16 yrs.	Anchylosis at an angle of 75°.	Intractable spasmodic movements of the semi-tendinosus, semi-membranosus and biceps muscles.	Bony union in 5 weeks.	8 inches, 3.2 ctm.	
11	"	Male, 8 yrs.	Caries.	Fistula formed.	Bony union in 10 weeks.	4 inches, 1.5 ctm.	Caries returned after two years.
12	"	Female, 24 yrs.	Caries.			3 inches, 1.2 ctm.	
13	"	Male, 11 yrs.	Caries.			4 inches, 1.5 ctm.	Refused to take nourishment. Died within twenty days of exhaustion.
14	Prof. Busch, of Bonn.	Male, 15 yrs.	Fungous inflammation of knee-joint.		Bony union within 3 yrs.		Second resection performed of sawn surfaces of femur and tibia; fistulous openings, however, still remain.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
15	Prof. Busch, of Bonn.	Female, 26 yrs.	Bony ankylosis at right angle.	Healing of soft parts completed in twenty-one days after operation.	Bony union, 9 months.		
16	"	Male, 19 yrs.	Caries of the lower epiphysis of the femur and the upper epiphysis of the tibia.		Bony union incomplete after six months.		One fistulous opening at end of six months.
17	"	Male, 24 yrs.	Fungous tubercular inflammation of the knee-joint.				Exuberant tubercular granulation from the wound. Amputation of the femur.
18	"	Male, 31 yrs.	Caries of the articular surface of the tibia and of the external condyle of the femur.		Uncertain if bony union ever took place.		Wound healed under antiseptic treatment. When patient was discharged three fistulous openings had occurred; patient could flex his leg.
19	"	Female, 20 yrs.	Caries of tibia and femur.	Pyæmia.			Amputation of femur; died five days after amputation.
20	"	Female, 19 yrs.	Caries of tibia and femur.		Bony union within six months.		Healing without fistulous openings.
21	"	Male, 20 yrs.	Fungous synovitis.	Abscess of the thigh.	Bony union within six months.		Fistulous opening which finally closed.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Healing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
22	Prof. Busch, of Bonn.	Female, 19 yrs.	Caries of the lower part of the femur following acute osteomyelitis.	Elevation of temperature during the first three weeks. Abscess of the thigh.	Bony union within nine months.		After five months spicula of bone removed from tibia; after eight months all fistulous openings closed.
23	"	Male, 37 yrs.	Bony ankylosis at right angle.		Bony union within four months.		Cure without any sign of fever; no fistulous openings; nine weeks after operation was able to use the limb.
24	"	Female, 8 yrs.	Fungous inflammation of the knee-joint.	Healing process very slow.	After 16 months no bony union.		General health very much improved after operation; fistulous openings after sixteen months.
25	"	Male, 44 yrs.	Caries of the knee-joint.	Compelled to amputate at the thigh after seven and one-half months; rapidly progressive tubercular phthisis.			Died; post-mortem showed the sawed surfaces carious and not a sign of union visible.
26	Dr. J. Brandt, Klausenburg, Hungary.	Female, 18 yrs.	Caries of the right knee-joint.		Incomplete union when patient was discharged. A new patella, the size of a walnut, had formed.		Amputation performed two years afterward. At the operation it was found that the femur and tibia were firmly united. Patient died seven months after amputation. At the post-mortem amyloid and fatty degeneration of almost all of the internal organs.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
27	Prof. August So- cin, Basle, Switzerland.	Female, 12 yrs.	Inflammation of right knee-joint with caries.	Wound did not heal, but filled with fungous granulation.			Five months after the operation amputation of the femur. Rapid re- covery.
28	"	Male, 4 yrs.	For two years fungous inflammation of the knee- joint, with suppuration and several fistulous open- ings.	At the operation tibia found to be sound and a resection of the condyles of the femur only per- formed; healing com- plete in one month.	Bony union incomplete; some motion in the joint.	5 ctm.	During convalescence the patient fell and sustained a fracture of the femur. Finally a good recovery; walks, with a support, consisting of a leather cap over the knee; he does not limp.
29	"	Male, 6 yrs.	Inflammation of the knee-joint, two and one- half years' standing. Leg badly nourished; posterior subluxation of tibia; limb strongly flexed.	No fever.	At ten weeks firm consoli- dation of the bones.	2½ ctm.	Drainage openings healed slowly. Patient walks easily with a light sup- porting apparatus.
30	"	Female, 19 yrs.	Inflammation of the knee of three years' stand- ing. Fistulous opening consequent upon punc- ture; knee-joint exces- sively painful; circum- ference 36 ctm.; slight flexion.	No rise of temper- ature following oper- ation.	After four and one-half months con- solidation im- perfect.	5 ctm.	Discharged after four and one-half months; walking with a supporting apparatus.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
31	Prof. Billroth.	Female, 6 yrs.	Tumor albus.	Gangrene of the borders of the flap followed by suppuration.	Firm bony union in 14 weeks.	3 ctm.	
32	"	Female, 17 yrs.	"	Wound healed by first intention.	No union.		Four weeks after the operation amputation of the femur performed.
33	"	Female, 4 yrs.	"	Healing by first intention, with the exception of some fistulous openings, which continued to discharge pus for several months.	Firm bony union after 9 months.		
34	"	Male, 9 yrs.	"	Wound healed by first intention. Subsequently caries; albuginaria.	No consolidation at the end of four months.		Patient not heard from after four months.
35	"	Female, 25 yrs.	"		Firm bony union within three months.	5-6 ctm.	

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
36	Prof. Billroth.	Female, 8 yrs.	Tumor albus.	Primary union of the wound. Caries subsequently occurred and new fistulous openings formed, which finally healed.	After eighteen months no bony consolidation, but firm fibrous union.	5-6 cm.	Final cure with use of limb.
37	"	Male, 23 yrs.	Contraction following chronic arthritis.		Firm union in ten weeks.	7 cm.	
38	"	Female, 18 yrs.	Tumor albus.		Union in seven weeks.	5 cm.	
39	"	Male, 7 yrs.	"	Caries recurred after rapid and uninterrupted healing.	No bony union at the end of ten weeks.	2-3 cm.	Patient lost sight of.
40	"	Male, 9 yrs.	"	Suppurative osteomyelitis and periostitis.			Death on the 10th day.
41	Prof. E. Albert, Innsbruck, Austria.	Male, 17 yrs.	Contraction of the knee-joint following fungous inflammation.	Integument about the joint, gangrenous in consequence of strong tension. Silver sutures removed in 5 weeks. Some rise of temperature for 39 days.	Firm bony union at the end of one year.	4-5 cm.	

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Resecting.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
42	Prof. E. Albert, Innsbruck, Austria.	Female, 14 yrs.	Disease of both knee-joints; paralysis.	Resection of both joints at the same operation. Secondary hæmorrhage on evening following operation. Fever.	Firm bony union.		Patient walks with apparatus.
43	"	Male, 30 yrs.	Fungous inflammation of knee-joint. Capsule thickened; lateral motion.	Fever after 2d day, gangrenous spots in skin. Wound healed by first intention. Four months after scrofulous ulceration. Patella subsequently removed.	Firm bony union.		
44	"	Female, 57 yrs.	Genu-valgum; lateral motion; considerable enlargement of the joint; pseudo fluctuation; no fistulous openings.	Fever and bronchitis, fungous granulations, osteo-myelitis. Secondary operation.	No bony union at date of report.		Patient able to hold her limb up, when the joint describes a slight angle.
45	"	Male, 30 yrs.	Chronic inflammation of knee-joint.	Fungous growth from wound. Erysipelas, fever.			After two operations, on account of fungous growths, amputation of the femur. Slow recovery.
46	Prof. von Bruns, Tübingen.	Male, 7 yrs.	Anchylolysis, spontaneous subluxation of tibia.	Abscess formed at sawn surface of tibia.	No union.		Amputation finally performed. Patient discharged, cured, within five months from first operation.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
47	Prof. von Bruns, Tubingen.	Male, 17 yrs.	Caries.	Healing delayed by subcutaneous abscesses.	Firm bony union in three months.	Shortening.	
48	"	Male, 12 yrs.	Knee ankylosed at a right angle.	No complications.	Union in two months.	1 ctm.	Patient walks without support in three months; heel and sole of foot raised.
49	"	Female, 13 yrs.	Anchylolysis of knee and outward rotation of leg.	Delayed union of soft parts; abscess formed at inferior part of wound.	Bony union at fifth month.	3-3 ctm.	Patient discharged, walking without support.
50	"	Male, 35 yrs.	Fungous inflammation of knee-joint.	Several abscesses originating in wound.	No bony union.	10 ctm.	Patient discharged, cured, in seven and one-half months, wearing artificial support.
51	"	Male, 12 yrs.	Knee ankylosed at a right angle.	No complications.	Bony union in two months and seven days.	6-5 ctm.	Patient discharged in less than three months, wearing heel and sole of boot raised.
52	"	Male, 19 yrs.	Anchylolysis of knee-joint, posterior subluxation of leg.	No elevation of temperature.	After seven months, union.		Patient left, wearing a plaster-of-Paris splint. At the end of seven months he was found wearing a raised heel and sole and working at his trade, as a stone-cutter.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during operation, while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
53	Prof. von Bruns, Tubingen.	Female, 17 yrs.	Ankylosis of knee-joint.	External wound healed within three months.	Bony union (time not stated.)	2.5 ctm.	Patient walks, after five weeks, with a raised sole and heel to boot and no other support.
54	"	Female, 15 yrs.	Ulcerative inflammation of knee.	Healing of wound delayed; numerous abscesses at the lower extremity of femur.	Firm bony union at 10 months.	4 ctm.	
55	"	Male, 15 yrs.	Fungous inflammation of knee-joint.	No increase of temperature.	Union in 8 weeks.		Discharged in two months, cured.
56	"	Male, 23 yrs.	Osteitis of internal condyle.		Union firm in 7 weeks.		Discharged, cured, at end of seven weeks; works at his trade as mason.
57	"	Male, 31 yrs.	Ulcerative inflammation of knee-joint.	Primary union.	Bony union in 3 months.	3 ctm.	Discharged, cured, at end of three months and eight days from date of operation.
58	"	Female, 32 yrs.	Ulcerative inflammation of knee-joint.	Healing process very slow.	No union.		Constant suppuration; amputation of femur finally performed.
59	"	Female, 13 yrs.	Ulcerative inflammation of right knee-joint.	No complications; wound healed kindly.			Patient died of nephritis and pulmonary oedema several weeks later and before bony union was complete.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
60	Prof. von Bruns, Tubingen.	Male, 9 yrs.	Fungous inflammation of knee-joint.	No fever; splints had to be removed three times during after treatment.	Firm union at end of 6 weeks.	2.5 ctm.	
61	"	Male, 18 yrs.	Left knee ankylosed at right angle.	But very slight elevation of temperature during first two weeks, none thereafter.	Firm bony union at end of 7 weeks.	3 ctm.	Discharged at eighth week, cured.
62	"	Male, 7 yrs.	Fungous inflammation left knee-joint.	Gangrene of borders of wound. Erysipelas, subsequent rapid healing.	Union when last seen, but not positive as to its being bony.	2.5 ctm.	Patient lost sight of at end of five weeks; flaps then not quite united.
63	"	Male, 24 yrs.	Fungous inflammation of knee-joint.	Considerable elevation of temperature; healing of soft parts slow.	Firm union at end of 6 weeks.	3 ctm.	
64	"	Male, 11 yrs.	Fungous inflammation of knee-joint.	Delayed union of wound; borders gangrenous; abscesses in thigh and leg.	Bony union in 4 months.	2.5 ctm.	Very useful limb.

No. of Case.	Operator.	Sex and Age.	Indications for Operations.	Complications during Operation, and Points of Note while Patient was Convalescing.	Union of sawn surfaces, whether bony or fibrous.	Amount of Shortening.	REMARKS.
65	Prof. von Bruns, Tubingen.	Male, 21 yrs.	Fungous ulceration of knee-joint. Fistulous openings leading into cavity of joint.	Rapid union of parts; very slight increase of temperature.	Firm bony union at end of six weeks.		Cure perfect, and patient walking about without support before seventh week.
66	Dr. Geo. R. Fowler, Brooklyn, N. Y.	Female, 40 yrs.	Chronic arthritis with subluxation backwards of the tibia.	Rapid recovery; no rise in temperature during whole period of convalescence.	Union very firm at third week; complete bony union at seventh week.	1 ctm.	Patient walks about without any support and with scarcely a perceptible halt in her gait.

Caries, 19; ulceration of cartilages (fungous inflammation), 17; deformity due to former disease in knee-joint, 16; osteitis of internal condyle, 1; chronic inflammation (tumor albus), 12; chronic inflammation of both knee-joints.

DISCUSSION.

DR. RUSHMORE said all modern cases of tabulation involve cleanliness. But they may be treated antiseptically and yet not according to antiseptic surgery. Antiseptic surgery is one thing, and Listerism is another. It is a kind of antiseptic treatment. But when one resects a knee-joint, he may treat it antiseptically without going to the trouble necessary to Listerism. This is an important point. We are apt to confuse Listerism with antiseptic treatment. When a drainage tube is used, or when there is a thorough drainage, he thought that in the majority of cases quite as good a result would be obtained as if Listerism were employed. In the cases that he had treated, using the drainage tube, the result had been very favorable, with movement of the limb. Of course, after such an authority as Lister, he thought it worth while to try his plan. The point is not whether you get as good results with Lister as you do without, but whether you do as a rule. He could recall two or three cases where no such trouble as Listerism involves was necessary. In one case, a man's toe had been cut, and it hung by a simple skin flap. It was reapplied and simply kept in position by adhesive strips, and the wound became perfectly well, and the patient has a moveable toe, without any Listerism. Another case was that of an Italian laborer, who had a compound fracture of his nose, and the scalp was thoroughly loosened and pretty thoroughly filled in with gravel and dirt. This was thoroughly washed out, and with the ordinary treatment the wounds healed perfectly well, with a minimum amount of surgical fever; exactly as good a result as if Listerism had been used. The Doctor thought that while Listerism has value, it has a good deal of trial yet to undergo before it is established on the basis that Mr. Lister claims for it.

DR. NORTH inquired of Dr. Fowler whether, in his investigation of this subject, he had found any reported cases of poisoning from absorption of carbolic acid. His impression was that such cases could be found. He thought that some cases that had been set down as absorption of pus had really been absorption of carbolic acid, with severe fever, and in some cases death, as a result.

DR. FOWLER replied that two cases of carbolic acid poisoning had been reported to his knowledge, and they were both from Billroth's clinic. But Billroth is not an enthusiast in Listerism, and he attributes these two deaths to carbolic acid poisoning. But in the tabulated record, under the head of remarks, it is observed with regard to one of the cases: "This patient died comatose at the end of twenty-four hours," without stating just what symptoms were present of carbolic acid poisoning. Dr. Fowler cited the following case: He had been called to see a patient who had been the subject of a recent abortion, and up to the fourth month the placenta had been retained, and the discharge from the vagina was of a horribly putrid character. Her pulse was prebably 200 to the minute, and he did not know how many he had lost in the count. He ordered for this patient a mixture of glycerine and quinine. Her drunken husband gave her, by mistake, a tablespoonful of a fifty per cent. solution of carbolic acid, and she swallowed it. He was called to see her shortly afterwards. She was suffering very greatly. He paid his entire attention to her stomach, and let her other trouble alone. The next morning her pulse resumed its normal character, and she made a rapid recovery. He had no knowledge that she ever suffered anything referable to her carbolic acid dose. In another case where a similar blunder had been made, there was a total suppression of urine for twenty-four hours; but whether that was due to the carbolic acid he did not know. But the cases of poisoning are so rare in surgical practice that it is not worth while to take them into account against Listerism.

DR. SHERWELL stated that he had used injections of carbolic acid, in one case with a child seven months old, and saw no specific effect of the drug whatever. He said he would like to record his non-belief in Listerism. He believed simply in neatness and scrupulous cleanliness.

DR. SMITH said he thought the Society were indebted to Dr. Fowler for a very interesting and instructive paper. He had not heard the whole of it, but he thought perhaps he had got the gist of it. He said there was no doubt in his mind that Listerism was a very great advance from anything which had preceded it. He thought the record that Lister furnished was a very excellent one, and that we should prize it highly until we get something better. In regard to the absorption of carbolic acid in Lister's method, he did not believe it was possible. He had seen a great deal of carbolic acid used, and had used it himself, in cases of confinement, after the birth of the child, with very marked benefit, and he had never seen any harm from it. He did not believe that its use in this way was productive of death, as stated by Billroth. When that is proved to be a fact, then it is time for us to consider that it is objectionable to that extent. He renewed his expressions of indebtedness to Dr. Fowler for his paper.

DR. WILSON inquired of Dr. Rushmore what were the initial symptoms of carbolic acid poisoning.

DR. RUSHMORE replied that he had never seen a case of carbolic acid poisoning. He knew of a case where carbolic acid had been used as an enema in the case of a child. About ten minutes after it was used the child became cold and began to vomit, and finally had a convulsion; and the mother volunteered the information, about two hours afterward, that she noticed an odor of carbolic acid in the urine. Aside from this, he had no personal experience of the symptoms of carbolic acid poisoning. He believed the odor and the smoky color of the urine were generally present in such cases.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A regular stated meeting of the Medical Society of the County of Kings was held at 398 Fulton Street, Brooklyn, on the evening of Tuesday, August 17th, 1880, the President, Dr. Charles Jewett, in the chair, Dr. J. H. Hunt, Assistant Secretary, acting as Secretary.

The meeting was called to order at 8:15, with about 50 members present.

The minutes of the previous meeting were read and approved.

The following propositions for membership were made:

Dr. John Willard, by Dr. Chase; Dr. Louis F. Criardo, by Dr. Reynolds.

The Council recommended the acceptance to membership of Joseph E. Smith, M. D., Jefferson College, 1875.

The Society then listened to the first paper of the evening, entitled "Calcium Salicylate in the Serous Diarrheas of infants," by Dr. A. Hutchins.

It was debated by Drs. Chase, Read, Segur, Kretzschmar, Gilfillan, Sullivan and the author.

DR. G. R. FOWLER read the second paper, entitled "Antiseptic Excision of the Knee-Joint, with table of Sixty-nine Cases."

It was debated by Drs. G. K. Smith, Segur, Rushmore, North, Gilfillan, Sherwell, Wilson and the author.

THE CHAIR announced that the following named gentlemen were duly accepted to membership in the Society: Drs. S. J. Sørensen, A. G. Meyenburg and G. A. Cardwell.

DR. KRETZSCHMAR made the following motion: Moved, that hereafter every author of a paper published in THE PROCEEDINGS, be entitled to — copies.

Motion not seconded.

On motion, adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρωποῖσι, κακῶν θελκτῆρ' οδυναῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—THE REGISTRATION OF ALL PHYSICIANS is compulsory under the Act published in THE PROCEEDINGS for August, and must be attended to before October 1st. The Book for Registration is at the County Clerk's office, and the fee fixed by law is 25 cents. Before registering, an affidavit setting forth certain facts must be sworn to and filed in the County Clerk's office. A form of affidavit is given below. It can be sworn to before a Notary Public or a Commissioner of Deeds. Each can prepare the affidavit for himself. Those living outside the city must omit the words "City of Brooklyn," and swear before a Notary. Those practicing under a "License," must substitute that word for "Diploma."

County of Kings, }
City of Brooklyn, } ss.:

..... being duly sworn, says
that he resides at, that he is
years of age, that he was born in the of,
State of, and that he is now, and since the
year has been, lawfully engaged in the practice of physic
and surgery in Kings County, having been authorized so to do
by the laws of N. Y., in force at that date, by virtue of author-
ity conferred on him by diploma from a duly chartered medi-
cal college ; said diploma having been granted him by
..... and dated
.....

Sworn before me
this day of, 1880.

—THE INTERNATIONAL SURGICAL RECORD (weekly) is a new aspirant for public favor. It is to be devoted to surgery exclusively, and to furnish complete transactions from foreign surgical literature. An In-

INTERNATIONAL TRADE DEPARTMENT is to be devoted to obtaining all the novelties in materials and appliances. The editor is Dr. Achilles Rose, No. 2 Chambers Street, New York City, and the subscription, \$5.00 per annum.

—A DOCTOR tells with pardonable pride how, being called in at the début of his career to a consultation with an eminent prince of science, he had insisted, despite the opinion of his famous senior, that the patient had an incurable affection of the heart.

“And what were my delight and pride,” he says beamingly, “on learning three days later that my patient had gone off precisely as I had declared he would.”

—HIGHER MEDICAL EDUCATION.—A special committee, in a report on medical matters to the Supreme Lodge, Knights of Honor, say that among the certificates of death “we found a death recorded as caused by ‘organic duodenum of the heart,’ a peculiar form of cardiac disease of which your committee have no knowledge; one caused by ‘dysphagia, on account of closing the glottis,’ and we are left to conjecture whether or no it was done voluntarily, by due process of law, or a dispensation of Providence; another was caused by ‘congestion of the brain *and* falling from a building;’ the primary cause of death we are unable to determine; another, in a little more than three months after initiation, died of the following onslaught of maladies: “inflammation of neck of the bladder, acute bronchitis, pleuro-pneumonia, inflammation of the left ear, nephritic trouble and functional cerebral trouble.” ‘Trouble’ enough to kill an entire Lodge.”

—MONTREAL GENERAL HOSPITAL REPORTS, VOL. I., 1880.—This excellent volume of Reports is the first issued from a Canadian Institution. The editor, Dr. Wm. Osler, contributes a very elaborate and interesting Pathological Report, as well as an instructive paper on Cardiac Abnormalities. Papers on Leucocythemia, Anatomical Abnormalities, Occipital Meningocele; and Lister’s Antiseptic method, as practiced in the Hospital, are portions of this valuable volume. A number of illustrations accompany the text.

—ENTERIC FEVER FROM MILK.—DR. JAMES CHRISTIE, of Glasgow, Scotland, investigated the cases of enteric fever that occurred last April in one of the districts of Glasgow. Ninety-two cases occurred in a population of 1,242, and 86 of the cases were traced to the milk from one dairy. The report of Dr. Christie is a fine showing of exact scientific work.

—PERSISTENCE THROUGH LIFE OF THE SOMATIC ELEMENTS.—DR. WOODSIDE, of this city, contributes a short article to the *Journal of Nervous and Mental Disease*, for July, 1880, in which he outlines an argument against the common teaching that our bodies are “continually undergoing what may be called physiological decay.”



SEE PAGE 262.

— THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The September meeting will be held on the 21st, at which there will be presented the following papers:

Report of the Committee on Hygiene.

Prophylaxis of Puerperal Convulsions, by Dr. B. A. Segur.

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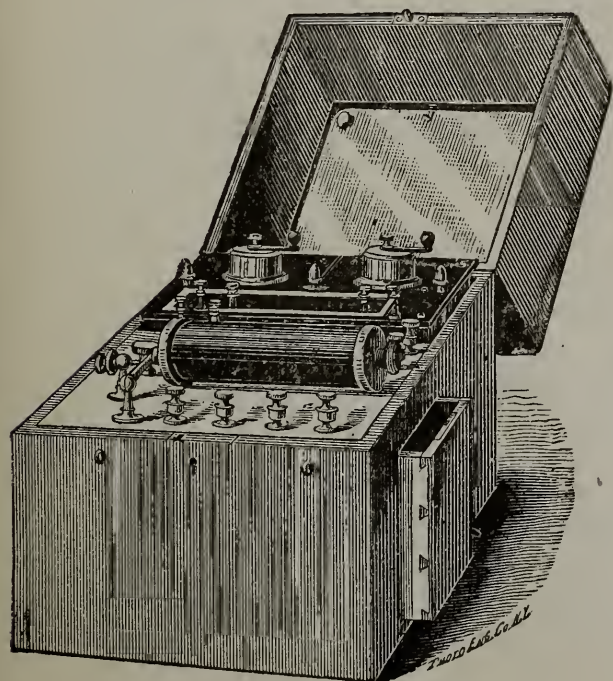
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LONDON LANCET.

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RANKING'S ABSTRACT OF THE MEDICAL SCIENCES.

Vol. 2, No. 1.—January to June, 1846.	Vol. 39.—January to June, 1864.
No. 16.—July to December, 1852.	Vol. 40.—July to December, 1864.
Vol. 36.—July to December, 1862.	Vol. 41.—January to June, 1865.
Vol. 37.—January to June, 1863.	Vol. 42.—July to December, 1865.
Vol. 38.—July to December, 1863.	Vol. 43.—January to June, 1866.

MEDICAL AND SURGICAL REPORTER, (PHIL.)

1 to 101, inclusive, 103.

 Revised to September 2, 1880.

ADDRESS

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PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

OCTOBER, 1880.

No. 8.

PROPHYLAXIS OF PUERPERAL CONVULSIONS.

BY AVERY SEGUR, M.D.

At the July meeting of this Society the treatment of puerperal convulsions was brought to the attention of the members by a paper advocating venesection as a remedy. Neither the paper nor the subsequent discussion directed our attention to the subject of the prophylaxis of the malady.

The President of the Society has thought that this subject ought to engage our attention, and particularly that we should consider the allegation or claim "that uræmic convulsions are extremely rare in puerperal cases that have been properly watched and treated from the first indications of kidney trouble before the labor; and that it is the duty of the physician to make weekly or fortnightly examinations of the urine during the latter months of gestation."

It is acknowledged that views of puerperal convulsions are changing, that the pathology is unsettled, and that on the most important questions in the treatment the authorities are not united in counsel. But it is not the intention to occupy the time this evening with a statement and comparison and argument on these varying views and doctrines and teachings.

Again, however careful, accurate and competent the observation of the physician may be of the cases that occur in his practice, it is obvious that his experience can teach him or others only in part, and quite possibly unfit him to accept the experimental knowledge of others.

So it has been thought that a better way in this matter would be to try to bring out and record in our PROCEEDINGS the large experience of our own members, that we should all think of what we know about puerperal convulsions with especial reference to prophylaxis, and be prepared, when called on by the President, to make a contribution at once.

To aid and assist in a methodical, comprehensive and logical treatment of the subject through the medium of an extemporaneous discussion, it has been thought expedient to have a preliminary review of the salient points, a brief presentation of the leading topics, a suggestion of the important questions.

This part is my undertaking, and whatever imperfection may be found in the questions I may raise, I make no doubt the answers will be according to wisdom.

DR. Fordyce Barker, in a discussion recorded in Transactions of the American Gynecological Society, for the year 1878, p. 196, says: "I have but rarely met with a case of puerperal convulsions in which the patient had previously been under preventive treatment." And he says he sees quite a large number of these cases every year of his life. That is in consultation practice in New York, I suppose.

Can it be said of the many cases of puerperal convulsions occurring every year in Brooklyn, that they do not have preventive treatment? If the answer is that our patients fall into convulsions untreated—why is it?

Do we have a correct impression about the frequency of this formidable complication of pregnancy? The only published statistics that are generally read, and go into all the text-books, show that convulsions occur in one out of 200 to 500 cases. Thus Churchill, in a table of the practice of the principal obstetricians of Great Britain, gives a total of 45,180 cases of labor with 92 cases of convulsions, or one in 491.

In France, as stated by Cazeaux :

"Les convulsions générales (éclampsie proprement dite) sont une maladie assez rare. M. Velpeau n'en a rencontré aucun cas sur mille accouchements qu'il a suivis à la clinique. C'est la bien certainement une circonstance exceptionnelle; car en consultant les relevés fournis par Madame Lachapelle, Merriman, Ryan, Pacond, De Bourg, etc., il y aurait à peu près un cas de convulsion sur deux cents accouchements."

In Vienna, Braun states that 44 cases of eclampsia occurred in 24,000 confinements. (1 in 545).

It has seemed to me from the number of cases which physicians speak of seeing in their practice, that they must be more frequent now than in the time of the physicians who have furnished the data for these old tables; or, perhaps, more frequent in our country than abroad. I have no statistics of the proportion of convulsions to

confinements, but I have been interested in the results of a calculation I have made, and which I offer simply as an estimate. We have published bills of mortality in Brooklyn for the four years 1873-4-5-6. The number of deaths from puerperal convulsions were, respectively, 20, 27, 32, 26, or 105 for four years, and an average of, say, 26 annually. If we knew the proportion of deaths to cases, and the total number of confinements or births, we could then determine the number of cases of convulsions.

Now, Barker, in "The Puerperal Diseases," illustrates the rapid improvement which has been made within a few years past, and the present comparatively successful treatment of this malady, by giving older reports showing 1 death in $2\frac{1}{2}$ to 4 cases, and then recent reports of Dublin and London experience, with the more favorable showing of 1 death in from 5 to 12 cases. Of his own experience in New York he says:

"Since 1855 I have had 7 cases in my own private practice, and I have seen 58 others in consultation practice, not including those that have been in this hospital; and in these 65 cases there have been 9 deaths, or a little over 14 per cent. I fully believe, with Dr. Davis, 'that this mortality will be still further reduced as that valuable agent, chloroform, comes more to be employed in suitable cases for its administration, and other indications are fully recognized.'"

"Since this lecture was prepared for the press, I have seen seven cases of puerperal convulsions, six of which were in consultation, and all of them recovered."

If the treatment in Brooklyn is equally successful, there are 8 cases of convulsions for every death, and ($26 \times 8 =$) 208 cases occur in the 17,000 (the number estimated for me by the Registrar of Vital Statistics, Dr. Wyckoff) annual births; or one in every 71 confinements. Such a proportion is so unexpected that we may hesitate to accept it, but even if we adopt the maximum of 1 in 5, we still show that in every 130 confinements we must expect to meet a case of convulsions—an opportunity, certainly, for a good many of our members to be practicing a preventive treatment.

But how many of these one or two hundred annual cases of convulsions give any warning or signal of the storm or premonitory symptom? I hope we may be instructed on this question to-night. I find that the very best clinical observers are the most positive in their assertion that in a very large proportion of cases the physician is forewarned, or at least may see the early indications, if he will carefully observe and study his waiting women. Cazeaux says of

"Phénomènes précurseurs.—L'accès d'éclampsie ne débute presque jamais à l'improviste: le plus souvent il est précédé de phénomènes qui peuvent en faire prédire l'invasion prochaine. Chaussier pensait même que dans les cas exceptionnels où les observateurs ne les ont pas mentionnés, ils ont été de courte durée, et, par cela même, méconnus ou inaperçus. Mais cette opinion est un peu trop absolue. Ces symptômes précurseurs manquent quelque fois."

But he immediately confuses this testimony by quoting M. Wieger as follows:

La fréquence comparée des prodromes diffère selon les périodes où l'éclampsie débute. Celles qui éclatent avant le travail, dit-il, ont des prodromes environ dans 40 pour 100 des cas; celles qui éclatent pendant le travail ou la délivrance ont des prodromes dans 30 pour 100 des cas; et celles qui éclatent dans les couches ont des prodromes dans 20 pour 100 des cas.

In the early years of the professional life of some of our still active members, they were taught (as I have read in their old books) that among the causes of epilepsy, were pregnancy, teething, worms, etc., thus confounding what we have now distinguished and separated in the important relations of nature, causes, prognosis and treatment—all unknown, unthought of in the identity of the convulsive phenomena.

Misleading and contradictory in the treatment of puerperal convulsions (epilepsy due to gestation or labor as they thought of it), prophylaxis was as confused and ineffectual. As knowledge of the ætiology of convulsive phenomena increased the convulsive diseases were differentiated, the term epilepsy came to be used in its present restricted sense, and as apoplexy, anæmia, uræmia, etc., were appreciated as causes, they gave name to the convulsive symptom, while the identical likeness of the convulsive seizure to epilepsy was indicated by the term epileptiform convulsions.

There is still in use the term eclampsia to include the convulsions of children and *puerperæ*, but it appears to be passing out of the books, and the tendency is to speak of the latter as puerperal convulsions, the adjective puerperal being extended beyond its etymological sense to include the period of gestation as well as childbed.

I have alluded to the state of knowledge when the convulsions in question were called in effect puerperal epilepsy; what more intelligible idea is at the present time conveyed by the term puerperal convulsions? Do we mean that puerperality is the cause of the convulsion as apoplexy is of apoplectic convulsions? It seems to me more consistent with the knowledge we have gained, to regard all convulsions as a symptom merely, manifested by the nervous system; but, according to the known laws of nervous action, due to conditions centric or eccentric, as sudden anæmia from hemorrhage or vaso-motor spasm, or from reflex irritations, etc.; and just as we endeavor to connect the symptom, febrile movement, with the bodily state of our patient, say typhoid or malaria, or pneumonia, as giving rise to the abnormal heat of the blood and tissues, so should we go behind the frightful muscular distortions of a convulsion and the compelling nerve force and agency, to the morbid influences which can so excite the nervo-muscular phenomena.

And as we clinically consider age, sex—the indications of the associated signs and symptoms, and the previous history of organic or functional diseases that have culminated in a convulsion—so in the dead-house we pursue our search beyond the convulsive centres of the nervous system, in every direction, brain, circulatory system, kidneys, etc., for sufficient lesions.

This point seems to me so important that I desire to quote as the text of all that I have to say, the following from the “Principles of Medicine.” p. 453, Williams.

Prophylaxis treats of the means by which particular diseases can be guarded against, and is rationally founded on a due knowledge of the causes, nature and tendencies of disorders, and of the various circumstances in regard to diet, regimen, residence, and medicine, which are capable of removing the causes or of counteracting their operation.”

And to the same purpose are the following concise and suggestive sentences from the “Manual of General Pathology,” p. 37, Wagner:

“What we know about the casual conditions of internal diseases is not for the most part about causes in the strict logical sense of the word, about *causæ sufficientes*, which of themselves must, under all conditions, produce a definite result, but is about complex conditions, under whose influence, with more or less frequency, certain diseases appear.

“From a rightly conceived ætiology flows in a natural way the *Prophylaxis* of disease.”

In 1843, Lever (Guy's Hospital Reports) first traced the connection between the epilepsy known to the accoucheurs and albumen in the urine. But seven years later Churchill calls all convulsions occurring in pregnancy and childbed “Epileptic,” excepting the comparatively rare variety, apoplectic convulsions. He treats at length of the causes, but makes no reference to albuminuria under that head. Among symptoms, and as if it were as much arising from what is passing as the biting of the tongue, he quotes, but without comment, from Lever as follows: “I have carefully examined the urine in every case of puerperal convulsions that has since come under my notice, both in the Lying-in Charity of Guy's Hospital and in private practice; and in every case but one, the urine has been found albuminous at the time of the convulsions.” Think of that short sentence for one moment! Nearly all we know, all we think of doing for our puerperal patients in the most terrible accident of their state grows out of it, is prompted and directed by it, yet it stands on the pages of an authority of only thirty years ago, waiting for interpretation, like the handwriting on the wall which the Assyrian King read but understood not.

There is a form of Bright's disease of the kidneys, which was first studied in connection with scarlatinal dropsy, and which has been subsequently, and by the work of many observers, connected with numerous condi-

tions as causes. It is found with other zymotic fevers, with diphtheria, etc. Exposure to cold is a very frequent cause. This disease is known as acute parenchymatous nephritis, acute Bright's disease, inflammatory dropsy, etc.

The kidney in this disease presents defined changes—an established morbid anatomy. The symptoms, progress, etc. of the disease are characteristic.

Its access is marked by swelling of the face, hands, lower extremities; genitals, etc. The quantity of urine is small, its color dark, its contents albumen, blood globules, casts, renal epithelium, etc.

At any period of its course uræmia may be developed, perhaps suddenly and the earliest symptom.

“The uræmic phenomena are due to the retention in the blood of the excrementitious matters of the urine. They consist in a train of nervous symptoms, headache, vomiting, diarrhea, convulsions and coma—which are frequent incidents, and much to be feared in acute Bright's disease. They usually follow an excessive diminution or suppression of the urine from the increasing obstruction in the kidneys.”—Roberts, p. 374.

Not to pursue symptoms further, the effect or resulting condition is always deficient renal function. Pregnancy is a cause of this form of renal disease, with its accompanying symptoms and insufficient excretions. How frequent and important a cause may be seen by the following:

“While women who are the subjects of Bright's disease may become pregnant, and pregnant women are liable, like other people, to contract Bright's disease from any of its ordinary causes, there is a third category in which pregnancy has really caused Bright's disease.

“The Registrar-General's reports furnish some valuable evidence on this point.

“In the five years between 1857 and 1861, 6,220 deaths were registered from Bright's disease. Of these 3,699 were males and 2,521 females, the relative proportion of the sexes at all ages being 68 females to every 100 males; but during the child-bearing period (from 20 to 45) the proportions were 80 women to every 100 men, while after the age of 45 the proportionate mortality from Bright's disease fell to 59 women for every 100 men. The only conclusion that can be drawn from these statistics seems to be that pregnancy is a fruitful cause of Bright's disease.”—Reynolds, *Sys. Med.*, Vol. V., p. 495.

Cazeau states that since attention has been directed to the connection between albuminuria and eclampsia, the subjects of the former are of all pregnant women the only ones, a few excepted, who have convulsions. He insists strongly that the cases where albumen was not found were not well observed, and says that in eight years in his own practice he had not met an exception.

Cazeau published his views as above in 1858.

Since then many cases of convulsions have been published in which

albumen was not found at all in the urine, or not until after the convulsions had occurred. A well-observed case so far as albuminuria is concerned is reported in the April number of the *American Journal of Obstetrics*, p. 403. It was in the Philadelphia Hospital; as the head was being delivered a convulsion occurred; the house physician immediately drew a few ounces of urine with the catheter; there was no albumen. In eight hours nineteen convulsions followed, and then albumen and casts were found.

But it is an established proposition that in a very large majority of cases albuminuria exists.

The importance of albuminuria to the puerpera, and its significance to the physician, are to be measured by the proportion in which it occurs in all puerperæ.

If recent statements are sustained the future appreciation of this symptom must sink very low. Thus Richardson in *Gynecological Transactions*, Vol. III., p. 178, 1878, states that

“Albuminuria has unquestionably played too important a part in the discussions of the various diseases incident to pregnancy. It has even come to be considered by some writers as the immediate and certain precursor of an attack of eclampsia. A more careful consideration of the subject, however, shows that the presence of albumen in the urine of a pregnant woman is by no means so rare an occurrence as some authors would lead us to suppose. An examination of the urine of the last one hundred and fifty-six patients admitted to the Boston Lying-in Hospital has shown albumen in varying amounts to be present in sixty-four, or nearly forty-one per cent.”

No particulars are given of the method of testing for albumen, the number of testings of each woman, or the period of the pregnancy.

The following quotation is from the *American Journal of Obstetrics*, July, 1880, p. 652, and confirms Richardson, but gives the matter a very different aspect:

“From statistics of Mayer and Leitzmann, and from his own examinations, R. Moericke finds that, while albuminuria during pregnancy is rare—some 4.71 per cent.—during *labor* it is frequent—40.78 to 43.70 per cent.

In 100 healthy women, in which labor was normal, or at least went forward without rise of temperature, the urine was examined during labor, and for a few days afterward. In 37 albumen was found, and in 13 casts. The latter (13) cases are given in detail. In all but two the appearance of albumen and casts dated from the time of labor. In two there existed some kidney affection earlier in the pregnancy. Cases 8 and 13 are interesting, because of labors prolonged several days by narrow pelvic straits. In No. 8 there was upon the *first* day of labor a little albumen and no casts, in No. 13 neither albumen nor casts, while on the third day in No. 8, and the second day in No. 13 there was a high specific gravity, much albumen, bloody and hyaline and granular casts. M. considers that in these cases the nephritis was positively caused by the act of parturition, and quotes the experiment of Weissberger and Perls on animals, and the kidney complications of cholera, to show how few hours are necessary to produce such action in the kidneys. He believes that casts do not necessarily imply

structural change in the kidneys, but that they certainly point to some disturbance of nutrition and function. The cases he details would seem to substantiate the latter view, for, in very few days after delivery, in all the cases, the casts and albumen disappeared. Hence he heads his article renal *affections*, and not necessarily *inflammations*. Ten of the thirteen cases were primiparæ; in the other three the labor was slow and difficult—another strong point in favor of his theory.

“From all these facts the author draws his before-stated conclusions. He closes by stating that all therapeutics are of no avail in these cases, but that all the indications are in favor of premature delivery, especially since Hofmeier has shown that in such cases 57 per cent. of the children are still-born, and of the remainder, many are born at the end of the eighth or beginning of the ninth month.”

According to this statement it is only in labor that the high albumen rate is found, and it loses none of its importance in pregnancy.

Neither the presence or absence of albumen in the urine is decisive of the question of nephritis; and, still more, we are not justified in suspending our search and watch for uræmia, because of its absence. Indeed we may find uræmia without nephritis.

I hold the opinion that uræmia is far more frequent a condition or occurrence than is generally understood. I think I often enough see it in hospital patients, and now more frequently recognizing it in anomalous cases, where formerly I could not make a diagnosis, I now see confirmation of the interpretation of symptoms in the prompt favorable effects of a treatment directed accordingly.

I may say that the subject of uræmia is very imperfectly and briefly presented on its clinical side in the books that I have read, while pages are given to the diffuse discussion of ammoniacal and other theories to account for the fact that a considerable failure of the function of the kidneys is dangerous to life.

Such pronounced phenomena as loss of vision, intense headaches, nausea and vomiting, diarrhea, perversions of intelligence, somnolence or restlessness, stertorous sleep or wakefulness, coma and convulsions, are catalogued over and over again—the barest anatomy of the subject without form or color.

Uræmia and convulsions, then, are common to puerperal nephritis, as to scarlatinal or other nephritis.

Rosenstein, quoted by Bartels, states that eclampsia occurs in one-fourth of the cases of nephritis of pregnancy.

I cannot find any statistics of the frequency of convulsions in other forms of nephritis, probably 1 in 6 or 8.

Certainly there is some factor present that determines a larger proportion of convulsions in puerperal women. Is it that factor which, in a few cases, is sufficient to produce convulsions of itself, when no evidence of structural or functural renal disease or disorder can be found?

Barnes, treating of the convulsive diseases of women, points out that the normal functions of the generative organs are convulsive in character, and Tyler Smith has elaborately shown by examples, that in a puerperal state the central nervous system is excited to convulsive discharges by many reflex irritations, as a loaded stomach, constipated bowels, distended bladder, and that the genital organs, eminently in labor, give rise to reflex convulsions; excessive liquor amnii, the pressure of the head in the cervix and vagina and vulva, obstetric manipulations, etc., affording examples. When such exciting causes are found, prophylaxis consists in removing the exciting cause, and a well-conducted case of labor must ever be guided by the controlling fact that we have to do with a physiological nervo-muscular, reflex-motor apparatus, acting with very great and even violent energy.

It is claimed by many, the ablest of obstetric physicians, that an efficient and persevering preventive treatment will in nearly every case conduct the pregnant and puerperal women safely through the threatening dangers of convulsions.

Others see no timely warnings or indication for preventive treatment, or set a narrower limit to a safe perseverance therein, and counsel an operative evacuation of the parturient canal wherever the child may be along its course.

They point to the statistics of cessation of convulsions with delivery as evidence that to remove the contents of the womb is to stop the cause of convulsions.

The subject has been recently discussed at length in the New York Obstetrical Society and elsewhere, and I hope it will receive here to-night the attention due to the most important matter connected with either prophylaxis or treatment.

In conclusion permit me to say that I know how inadequate a presentation of "The Prophylaxis of Puerperal Convulsions" I have made. My title would have better expressed my part in the design of this evening's work if entitled "What are the Factors in a Rational Prophylaxis of Puerperal Convulsions?"

I have stated them as I understand them, and leave it for the Society, in the discussion which is to follow, to question and assign their place and value.

To my mind they indicate that

1. We should study the convulsions of puerperal women as a symptom solely, as we do the convulsions of all other persons.

2. The convulsions of puerperal women are in almost all cases a symptom of insufficient performance of renal function—whether due to acute Bright's disease or to a disturbance simply functional. They are simply one of a uræmic train of symptoms.

3. Then the preventive treatment of uræmia is the prophylaxis of puerperal convulsions, as at the setting in of the uræmia it is very often too late to avert convulsions.

4. A pregnant as well as a parturient woman should be considered a subject for responsible professional care, and particularly be watched with reference to the function of the kidneys.

AN INVESTIGATION INTO THE CAUSES OF ALLEGED FUTILE AND BAD RESULTS FOLLOWING VACCINATIONS.

BY THE COMMITTEE ON HYGIENE OF THE MEDICAL SOCIETY OF THE COUNTY OF KINGS.

Your Committee, after considering various topics that might be of interest to the Society, chose the above, being led thereto by vivid statements in the newspapers as to the evil effects which had resulted from certain vaccinations in New York and Brooklyn, and because an anti-vaccination society in our sister city was issuing pamphlets containing views calculated to disturb the public mind.

It therefore became the duty of your Committee to inquire into the facts and elicit the truth—that is, so far as it had the power to do so.

In an interview with the President of the Anti-Vaccination League that gentleman stated that the Society was yet small—about 20 members. “Individually, he had not had time to attend to its interests. Personally, he had not examined into any of the so-called fatal cases of death from vaccinations reported as having occurred in New York. His opposition to vaccination was based upon general principles, and these were founded upon an analogy between the effects of introducing septic germs and vaccine virus into the human system.” For particulars he referred the interviewer to Dr. R. A. Gunn. He had investigated one case and found the facts substantially as set forth in the daily papers. The interviewer could not ascertain that any member of the League had investigated the cases or had paid any particular attention to the study of the quality and modifications of vaccine virus. Anti-vaccination statements had been obtained from their co-workers in England, and formed the basis of their information here—as is shown by a perusal of Dr. Gunn’s pamphlet entitled “Vaccination and its Evils.”

Vaccination is there regarded as an unmixed evil—with no recognized principles, and no safeguards governing the operation, and the statement is made that not “a single person has ever been protected from small-pox

by vaccination.”* “That the whole theory is founded on assumption and is contrary to common sense and all the known principles of physiology.”

The pamphlet conspicuously displays extracts from the testimony of the Anti-Vaccinationists before the Select Committee of the House of Commons in 1871, appointed to consider the Vaccination Act of 1867, but no mention is made of the testimony in favor of properly conducted vaccination of such experienced men as Sir Wm. Jenner, Sir Wm. Gull, Dr. West, Mr. Marson and Mr. Hutchinson, or of the fact that the Select Committee itself, composed of “representatives of all shades of opinion,” after eight sittings, during which arguments for and against vaccination had been thoroughly considered, reported:†

“That the cow pox affords, if not an absolute, yet a very great protection against an attack of small-pox; and an almost absolute protection against death from that disease.

“That if the operation be performed with due regard to the health of the person vaccinated, and with proper precautions in obtaining and using the vaccine lymph, there need be no apprehension that vaccination will injure the health or communicate any disease.

“That small-pox, unchecked by vaccination, is one of the most terrible and destructive of diseases as regards the danger of infection, the proportion of deaths among those attacked, and the permanent injury to the survivors; and therefore

“That it is the duty of the State to endeavor to secure the careful vaccination of the whole population.”

Among the statements before the Select Committee, of the “horrors of vaccination,” is the following: Rev. Hume Rothery testified. “A. B., vaccinated when a babe; family all perfectly healthy; cancer appeared on chin when 18 months old, and she lost the left breast by cancer at 13 years.” The interview with the anti-vaccinationists and the study of their literature gave your Committee no clue as to the real causes of futile or bad results, and deaths even, which are said to have followed vaccination. Of the fatal results vividly portrayed by the newspapers at the time—the New York Board of Health had knowledge of but four cases that could at all be so classified. No trace of other cases could be found in the city. These four cases were in children from the tenement population, all vaccinated with bovine virus obtained from the Health Department—three of them by Sanitary Inspectors. Of these three, in one case, the first cause of death was reported as diphtheria; the second erysipelas; the second child died from “abscesses, sloughing sore arm and septicæmia;” the third, apparently from septicæmia. Erysipelas occurred, and there was much sloughing at the point of vaccination.

Your Committee could not find the parents of the first child, they hav-

*Page 3 of pamphlet.

† P. 31—“Truth about Vaccination,” by E. Hart.

ing moved to some other tenement ; but Dr. Taylor, in charge of vaccination department, claims that that death was due to diphtheria and not to the vaccination, as the sore assumed the diphtheritic membranous coating. It was understood by your Committee that diphtheria was prevailing at the time of vaccination or appeared soon after.

In the second and third cases it is claimed that the sloughing, abscesses, etc., resulted from some inherent vice in the children which could not be foreseen. It is claimed by the mother of one of these that the child ($1\frac{1}{2}$ years old) was healthy and in good condition, with the exception of sore eyes (granular lids and conjunctivitis), for which it had been treated at the New York Eye Infirmary up to the time of vaccination. After the operation the arm inflamed ; she called in a physician, who told her that it was the result of "bad matter." The child lingered and died several months after vaccination.

Your Committee find that in almost every case where severe results have followed vaccination, some doctor has raised the cry of "bad matter," with no attempt at an investigation.

The fourth fatal (?) case was from erysipelas—vaccination done by a regular physician with bovine virus from the Health Department. The doctor told the investigator that he could not account for the result. The child was a healthy child—vaccinated with apparently the same virus as other patients were, on the same day, with no bad results.

So far, the information obtained by the Committee was in the main negative. There was no evidence at least that the vaccinations were savagely performed, as is undoubtedly the case sometimes, or that there was want of skill. The circulars of the N. Y. Health Department state that "it makes no difference at what time of the year vaccination is performed"—that "vaccination such as we offer will *always* prevent small-pox if attended to in time"—that "animal crusts as well as tubes are so thoroughly unreliable that the Department neither uses them nor offers them for sale"—but "a large supply of fresh and reliable humanized crusts of the first remove from the calf, from healthy infants only, are issued for use." "The virus from each calf is tested before using it to any extent, and if it does not show a proper degree of activity, it is thrown away."

"By following this plan the work of the Department is always to be relied upon." "The fact that the virus is thoroughly reliable at all times more than compensates for the extra cost connected with its propagation."

No deaths from vaccination had been reported in Brooklyn, but several instances where severe erysipelas resulted. All of these children, as far as could be ascertained, were vaccinated by one and the same physician, with virus furnished by the Brooklyn Board of Health. The

doctor in question believed that the erysipelas and sloughing were due to the contact of clothing with the sore.

The positive statements of the New York Health Department as to the reliability of the virus furnished by it; the equally positive statements by the vaccinators that the work had been properly performed by them; and the belief on the part of the parents that their children had been in good health up to the time of vaccination; all combined to turn the attention of your Committee towards the consideration of the subject of vaccine virus, its essentials and defects, the prevalent proper and improper methods of vaccinating, and the indications for the postponement or prevention of vaccination. To this end a number of physicians were interviewed—among them, Drs. S. S. Bogert, successor to Dr. Loines, of the Eastern Dispensary, New York; Dr. Frank Foster, the originator of bovine virus in New York City; Dr. Griffiths, of Brooklyn; Dr. Cutler, of the New England Vaccine Co., at Chelsea, Mass., and Dr. Henry A. Martin, at Boston Highlands, the introducer of pure bovine virus into this country. The want of time alone prevented your Committee from further interviews; but the information already obtained from the above-named gentlemen, with a study of vaccine literature, affords the following summary:

VACCINE VIRUS.

Drs. Martin, Foster, Bogert and Griffiths believe:

1st. That human virus, of the first three or four removes, if *properly* selected—*i. e.*, from healthy infants, the lymph being taken from the vesicles when clear, and no force being exerted so as to squeeze out the serum and blood of the surrounding tissues—is as effective as bovine, or, rather, “true animal virus,” as Dr. Martin prefers to call the virus descended from calf to calf; but such humanized virus is not readily obtained, especially in a sufficient quantity to meet a sudden large demand.

2d. That humanized virus deteriorates with each remove, while true animal virus rarely loses its strength.

3d. That “erysipelas is a disease whose occasional occurrence is inseparable from vaccination with humanized virus.” As Dr. Martin puts it: “I have very seldom known it to follow any other than a fine arm,”* and the records of the New York Dispensary and the Kine Pox Institution show an average of one case of erysipelas in every five hundred vaccinations with humanized virus; whereas *no case* has ever come to my knowledge out of thousands of persons known by me to be vaccinated with “true animal virus.”

* “A Few Words on Unfortunate Results of Vaccination,” by Henry Martin, M.D.—*Boston Med. and Surg. Journal*, Feb. 1st, 1877.

4th. That when erysipelas or bad results do occur, they are found in sickly children, or in the opposite class of strong, full-blooded children.

5th. That from one bovine vesicle but 20 to 50 points can be safely charged—the average being about 25—whereas, with the humanized vesicle a large number can be. Dr. Martin stated that from one vesicle upon a child he had been able to satisfactorily charge 1,800 points. The very fact that bovine vesicles discharge slowly and yield but a limited amount, is a temptation to unscrupulous men to squeeze out the serum and charge the points with it.

5th. Dr. Martin believes that there are distinctive vaccination corpuscles, as delineated by Burdon Sanderson in the 12th vol. of Privy Council Reports, and virus is not good without their presence. Dr. Griffiths has found vaccine corpuscles. Drs. Foster and Bogert do not put much faith in them.

6th. All believe in animal crusts, if properly selected and rid of foreign ingredients, dung, hair, dirt, etc. Humanized crusts are less reliable, for the vaccine sore is deeper in the child than upon the calf; there is more disturbance set up, and there is a loss of tissue. As to exactly what dermal tissues are involved, or if the connective tissue is destroyed, there is a diversity of opinion. Blood is generally found in such crusts, but may not in a dried state transmit disease, though one observer stated that the use of a crust, when other virus could be obtained, was culpable. Dr. Martin states that only the clear part of such a crust should be used; that all of the material (in reality a slough) clinging in a wedge shape to the centre and lower side of crust fallen off or removed before the time, should be cut away. That a typical crust, taken gently at the right time, has a depression on its under surface; that such a crust after lying in water for a time is nearly diaphanous.

The cow crust is larger, and has a different shape, owing to the different character of the vaccine sore in the animal. Crusts of either kind, if good and kept from moisture, can be used months after being removed. Dr. M. has used them successfully when two years old.

7th. All agree that lymph in tubes is uncertain, unless only a few days old; that bovine deteriorates faster than human, though no cause for this has been satisfactorily ascertained. Dr. Martin has found of tube lymph he has examined, only about 30 per cent. efficient. The observers agree that, generally, points kept in a cool place may work even when a month old; average time of service, 10 days; when kept in a warm room or in pockets, not more than 2 or 3 days; that the harder the virus becomes the longer it will keep. Dr. Foster has discarded the use of chloride of calcium, as its affinity for water rendered the virus too hard. They have observed that there is a great variation in the strength of dif-

ferent stocks of vaccine virus, that the effects are developed at variable times, that some "runs" of virus from the same stock will prove inert. Dr. Griffiths claims that all samples of virus should be free from blood corpuscles.

Dr. Martin says that bloody points are preferred by many propagators as more efficacious—*i. e.*: when the points are charged with the first virus that exudes from the animal vesicles, this first exudation being the strongest, but generally contains more or less blood. Dr. Foster did not believe that the blood made any difference, and Dr. Bogert agreed with this statement.

8th. As to the possibility of transmission of disease by "true animal virus," the gentlemen did not believe it was probable. Dr. Martin says it is an impossibility that a sick animal, or even when having diarrhea merely, will not "take" when vaccinated; that the inflammatory process set up by the breaking of a horn, in one instance, was sufficient to prevent the "taking;" that when the inflammation had subsided, a vaccination "took" successfully. "When an animal is vaccinated there is no apparent disturbance of the health. She does not suffer, but eats, drinks, ruminates, plays with visitors as usual." During the past seven years at least 5,000 bovine animals have been vaccinated in America; many of these have been most carelessly selected and improperly kept, and yet no disease has been communicated."*

9th. The gentlemen interviewed claimed that it has not been proven that vaccination, performed even with good matter, *after* a person has been exposed to variola, will necessarily ward off variola or modify it; that the variola poison may run its course. Dr. Bogert, in proof of this, stated that he was called to a house where one child was dying of variola. He vaccinated the remaining child, not then sick. The vaccination ran its true course, but the child died of variola. Dr. Martin said that he had had similar experience in several cases.

If this be so, the postponement of vaccination to times when small-pox is prevailing is to be deprecated.

10th. All of these gentlemen experienced in the propagation of animal virus claim that in order to insure success with virus every care is necessary in obtaining it from good original stock, at the proper time, in a gentle and proper manner, and that such virus, obtained in such a way, can *always* be relied upon, as Dr. Martin believes it is infallible. That the want of susceptibility is rare, but the maintenance of animals, etc., is attended with large expense—Dr. Martin putting the minimum expense at \$3,000—*i. e.*, if it is proposed to always have on hand a supply to meet emergencies.

* On Animal Vaccination, by Dr. Martin, Trans. A. n. Med. Ass. for 1877.

There are some 13 men, physicians and so-called "purveyors," who advertise to sell virus. There are many more who profess to furnish it, but who, when called upon to furnish a quantity, evade direct answers and ward off the needed supply to a "more convenient season." Drug stores sell virus, some from sources that cannot be ascertained. The pure "Beaugency stock," imported by Dr. Martin, has been drawn upon by new propagators for their first supply. Whether this stock in all cases has been kept up, or the vaccine has been mixed with humanized virus, or has been deteriorated by admixture with pus, dirt, dung, etc., etc., or has been supplanted by humanized vaccine of an inert or dangerous character, your Committee could not ascertain, as it did not have the power to go "behind the scenes." This duty must be delegated to another medical body, the National Board of Health, which alone has the power to enforce regulations and protect the public from inert or unsafe vaccine virus. Your Committee have reason to believe that such virus is sold, even to the army.

PROPER AND IMPROPER METHODS OF VACCINATION.

Let us glance for a moment at the modes of vaccinating—as bearing upon the inertness or dangers to result.

It may be assumed that all that is necessary in vaccination is to have pure virus absorbed into the circulation—that the removal of the epidermis *without* the appearance of blood is sufficient, before the virus is applied. But directions to vaccinators differ. One set says, "Rub in the virus gently." Dr. Martin has shown that the albumen in animal virus is less soluble in serum than that of humanized, and needs thorough rubbing. As proof of this he states that frequently physicians have returned to him points from a lot, some of which had failed to take. He has sent them back with the directions to rub in thoroughly and they have taken. Dr. Foster relates an interesting fact: A physician using animal virus furnished by the doctor wrote that after vaccinating a child in the morning the quill was thrown upon the floor. In the evening the father finding the discarded point, vaccinated himself—rubbing well. The father's vaccination took. The child's did not.

Thorough rubbing does not mean rough usage. The slashing cuts and the rough handling indulged in by some doctors are sufficient, according to competent observers, to induce inflammatory results.

At such times the flow of blood may wash off the virus and render the operation useless.

None of the patented contrivances for vaccinating, in the opinion of these observers, can compare with a rounded, pointed lance—providing that it is *kept* clean. Arm to arm vaccinations of several children and

that of a number at the same sitting are attended by dangerous risks. Has not disease been spread by *careless* vaccination?

Needles if frequently changed are advised, but perhaps still better are the ivory "points" themselves, which have sharpened edges.

Some doctors vaccinate two or more individuals with one point or quill of virus. Such economy is false and injures the cause of vaccination by failing at times to make virus take.

Woolen fabrics clinging to a vaccine sore will produce irritation and bad results. These are most likely to occur, according to Dr. Martin, when humanized virus is used, the resulting vesicle being tender and the crust more fragile than that from bovine virus.

While your Committee cannot endorse the opinions of the anti-vaccinationists as to the utter uselessness of vaccination, even when "duly and efficiently performed," as Jenner expressed it, and also agree to inaccurate statements, still they have reason to believe that inert and harmful vaccinations do occur.

As a result of their investigation they would lay down the following propositions:

1st. Vaccinate with only pure virus, animal or humanized, every child, when possible, before 5 months of age.

2d. The value of vaccination is lessened by lapse of time, so that re-vaccination is necessary between 10th and 15th year.

3d. It is wisdom to vaccinate before an epidemic occurs, as has been repeatedly urged by ex-Sanitary Supt., Dr. Segur, before the public is excited, and when virus can be readily obtained. As the danger of small-pox is great, children who might not be considered in proper condition for vaccination at other times should be vaccinated during the prevalence of variola.

4th. Children should not be vaccinated during an eruption of teeth, the prevalence of an epidemic of diphtheria, in the hot weather, if it can be avoided, or where there is any skin eruption.

5th. The causes of "spurious vaccination in the Confederate army," as investigated by Prof. Joseph Jones, are interesting in this connection, viz :

1st. Lowered vitality—scorbutic condition.

2d. From abnormal lymph, from persons previously vaccinated or having eruptive diseases.

3d. Scabs or lymph undergoing decomposition, long carried about the person.

4th. Mingling vaccine virus with that of true variola, as in persons having varioloid.

5th. Virus from persons having erysipelas, pyæmia, gangrene and suppurating wounds.

6th. Lymph-scabs, etc., from persons suffering from syphilis.

Gentlemen of the Society, if the assumption by your Committee be correct, that unreliable propagators of virus care more for the "almighty dollar" than they do for the purity of their vaccine; that, owing to the lack of any central body to arrange for and maintain pure and unadulterated virus, as is done in England by the National Vaccine Institution, there are unreliable dealers; that many of us have at least been led to believe that the "want of susceptibility" is the main reason why an uncomfortably large proportion of our vaccinations have failed, whereas Dr. Martin tells us that such plea can be *rarely* conscientiously advanced; if dispensaries not having an appropriation sufficient to purchase animal virus of pure quality rely largely upon gifts from various individuals—careless as well as careful ones; if your Committee believes all this, as it certainly does, and further, that with proper management, the purity and reliability of vaccine virus can be assured in the future, as are the same qualities in Dr. Squibb's medicinal preparations at the present time, it becomes a duty for your Committee to do what it can, through you, towards the furtherance of such an end.

The following resolution is therefore respectfully submitted for your action:

Resolved, That the Medical Society of the County of Kings, in view of the importance of pure and reliable vaccine virus being always attainable, do petition the National Board of Health to regulate and control the sale of vaccine virus.

Annexed is a tabulated statement of the results of some examinations of vaccine virus made by Dr. N. B. Sizer, of the Committee:

MICROSCOPICAL EXAMINATION OF VACCINE.

58 specimens as follows:

20	fresh lymph.
1	cone.
15	ivory slips, <i>my charging</i> .
20	" " commercial charging.
2	crusts.

58

In twenty specimens fresh lymph, only three contained the shining micrococci (so-called "vaccine corpuscle"). In eleven out of the twenty, despite care, red blood corpuscles were detected, absent *only* when the *spontaneously exuding* lymph alone was used; any attempt to increase the yield by pressure or any manipulation being apparently invariably followed by slight hemorrhage, perhaps microscopic only.

The fifteen slips of my own charging were then examined. Nos. 1 to 5, inclusive, charged with *freely* exuding lymph, contained no red blood cells.

Nos. 6 to 15, inclusive, where slight pressure was used as a "persuasive," *all showed red blood cells.*

Twenty slips (commercial) of various sources, showed eight with red cells, one had enough blood to show *pink* color, and this was a *humanized* slip !

In none of the quill slips have I been able to demonstrate any so-called "vaccine corpuscles," although the cells were easily made out.

The *cone* mentioned was largely composed of *débris*, whether accidentally or intentionally introduced is unknown.

Experiments were made to ascertain if the slips had been kept in proper condition as regards temperature, moisture, etc.; but as the results were largely negative and unsatisfactory, a detailed account will be here omitted.

Your reporter would sum up his results as obtained by his researches as follows:

1. The presence of blood in vaccine lymph is easily detected by the microscope.
2. Blood is a frequent contamination of commercial lymph in a dried state.
3. Its presence is probably due to the effort at obtaining as *large a portion of lymph as possible.*
4. Its presence is highly dangerous in humanized lymph, but probably of no account in that derived from the calf.
5. The so-called "vaccine corpuscle" has not been found in *quantity* or at *times* sufficient to render it undoubtedly a specific appearance due to the presence of vaccine alone. It appears to be merely a micrococcus.
6. Vaccine cones, as now offered in our market, are undesirable forms of transporting lymph.
7. The price of vaccine slips appears to determine the heaviness of the charge and the presence or absence of red blood cells.
8. Too much care cannot be taken in the charging of slips. Here, as elsewhere, the best is always the cheapest.
9. The crusts which I have examined consist largely of necrosed tissue, and hence would be likely to do damage if used indiscriminately. Probably only the thin, transparent top layer, is usable.

JEROME WALKER,
N. B. SIZER,
BENJ. EDSON,

Committee on Hygiene.

DISCUSSION.

DR. WHITE related a case which came under his observation while upon the Pacific coast.

In the year 1852, during a very severe epidemic of small-pox in San Francisco, he was called to a case of small-pox on the third day of the eruption. The patient was a girl, aged 15 years. When he first went into the room he saw what the disease was, and turning to the mother, who was a very poor woman, and there were six children, he asked her if the child had been vaccinated. She said no—none of them had. The family came to San Francisco a few months previously from Sidney, Australia, which perhaps accounted for it. They lived in two small rooms, and this girl had the disease in its severest type. He vaccinated the remainder of the family, and it “took” splendidly, and still occupying the same rooms with the girl during the whole period of her sickness and recovery, *not one of them had small-pox*, so that here was a family who were fully exposed, were vaccinated, and were all saved.

As regards humanized virus, he said that during a residence of fifteen years on the Pacific coast he never used any other—no bovine virus during all that time. All the ranche-men were vaccinated with humanized virus, a great many times removed from the bovine, and still he never saw or heard of bad results. He is favorably disposed towards bovine virus, and always procures it when he can, not especially because it is safer, but because the parents think it safer. If there are any accidents, people are not so likely to blame the physician.

DR. KRETZSCHMAR said that while others had mentioned the fact that they had never seen unfavorable results following vaccination from either humanized or bovine virus, yet he was one of those unfortunate individuals who had seen bad results follow. In May, 1878, he vaccinated three children with humanized virus. One child got along first rate, did well. Both the others developed soon after vaccination the first symptoms of erysipelas. One died and the other recovered. That was the last time he used humanized virus, as the erysipelas occurred in two cases out of three, and as the child who died was previously healthy—a first-born, healthy child, from healthy parents; and as there was no other explanation, he felt warranted in considering that the vaccination had considerable to do with the disease. He thought it strange that the committee could not find a record of the case in the Board of Health office, as it had been reported as a case of erysipelas following vaccination. Since that he has obtained bovine virus from the New England Company, and has used it with most satisfactory results. It comes on ivory slips. He had used vaccine occasionally six weeks after obtaining it, and with satisfactory results.

DR. H. N. READ thought there was in the report of the committee an important omission, namely, the influence not only of diphtheria on vaccination, but of the exanthemata generally when they are epidemic. In his estimation this is a most important consideration. He had had some unfortunate experience in this regard; as, for instance, cases of erysipelas directly traceable to scarlatina. Vaccination when measles prevails is dangerous. He had an unprecedentedly large opportunity for seeing bad results from vaccination, because of his connection with the dispensary. As cases turn out badly and trouble results, people abandon their physicians and bring their children to the dispensary and show the result of what they call bad matter. He had seen many of these cases following vaccination by the Board of Health, and they are all done with bovine lymph. He had seen many bad cases, so much so that it had at-

tracted the attention of Dr. Raymond. He had seen as many as fifteen cases in one house. In his own practice he has had erysipelas following vaccination with bovine virus. He has had very little trouble from humanized virus. Martin claims that erysipelas never follows bovine virus, but the dispensary experience of the speaker above related disproves that. Vaccination was done with all necessary precaution, and yet very bad cases of erysipelas undoubtedly followed from what was considered good, pure bovine virus.

DR. SEGUR desired to thank the committee for himself for the labor they had expended in this important matter. He was especially interested and obliged for the personal testimony which they had given from the four or five "experts" on this subject.

DR. LA ROE, from Greenpoint, believed that many of the so-called bad results from vaccination existed, to quote a popular phrase, "in the mind." He quoted several instances which did not, upon examination, bear out the accusation. He had been accused himself, while in the service of the Board of Health, of using bad matter in a particular case, when, upon investigation, it was found that he had not vaccinated the child at all! But it had been vaccinated by some doctor, and "it tuck the disease!" He differed from the gentleman before him (Dr. Read) as to the effects of bovine virus. He had visited nearly every case which he had vaccinated, and the result is good—it is 100 per cent. Referring to the statement of the committee regarding the period at which re-vaccination ought to be performed, he recalled the fact that some years ago the *New York Medical Journal* published the observations made upon Blackwell's Island, the conclusion of which was that in the institutions on that island small-pox had never been known to follow vaccination within three and one-half years.

He believed it to be the duty of this Society to spread that report, believing it will have great effect in preventing the spread of small-pox among the community.

DR. READ wished to correct the impression which might appear to be lodged "in the mind" of the previous speaker regarding his use of bovine and humanized virus. He meant to say that even with the use of bovine virus we have serious results unless proper precaution is taken. He believed in the use of bovine virus, and always uses it where he can. His remarks regarding the Board of Health vaccinations were not intended as a reflection. He merely stated this as a fact, and he himself called the attention of Dr. Raymond to it. He had seen fifteen cases in one house on Pacific Street, every one of them showing bad results following vaccination with bovine virus.

In reply to a question of Dr. Segur the speaker specified the results as erysipelas, abscess, ulceration extending some distance down the arm, eruptions of the skin, etc. The children as a general thing were unhealthy.

DR. LA ROE did not wish to be understood as saying that bad results could not follow bovine vaccination. He knew better than that—it can. He instanced a case of bad results following vaccination performed by the "scratching" process. His method was to draw as little blood as possible by using the quill, and then rubbing the virus in well.

DR. WILLIAMS depends upon humanized virus, because of the frequent failure of bovine virus.

DR. G. K. SMITH was surprised that a gentleman of the experience of Dr. Read should prefer virus one or two removes from the cow. He instanced a case from the experience of Dr. Otterson, showing the danger of depending upon humanized virus. During the war a soldier obtained a furlough and went home. While at home he was vaccinated for the prevention of small-pox, and about that time he also contracted the larger disease, which is the more terrible. He returned to his position at the post of

duty, and his pustule being at the proper stage, vaccine matter was taken from it and with it a portion of his regiment vaccinated. They were at the same time inoculated with syphilis, almost every one of them, and many of those who were thus infected were obliged to leave the service, being discharged because of unfitness for further duty. Now, if this subject is so important to us as this, it becomes necessary for us to know whether it is safe to use vaccine matter one or two removes from the cow or not, AND HE WHO KNOWS YOUNG MEN AS WE KNOW THEM TO-DAY knows that we cannot take the second remove and vaccinate our children with it with safety. "*If I vaccinate my child,*" said Dr. Smith, "*it must be with the bovine virus and not with any remove, first, second, third, fourth, or any other, from the cow.*"

Dr. WALKER related an incident in the army experience of Dr. Martin, regarding humanized virus among the darkies of Newberne.

Dr. READ, in reply to Dr. Smith, said he did not mean to say that he took virus one or two removes from the arms of syphilitic children, for he claimed to be able to distinguish between syphilitic and non-syphilitic people. He supposed in his reasoning that the people from whom crusts were taken were perfectly well. It is claimed that syphilis cannot be inoculated, by some of the best authorities.

Dr. SMITH asked Dr. Read how he was going to ascertain from the father of a child whether he ever had syphilis. And if he had, was he going to confess it? If these facts cannot be ascertained, there is a liability of obtaining bad virus.

Dr. BURGE asked whether it was not a noted fact that these gentlemen always tell the truth!

Dr. JEWETT suggested the question whether there is any evidence to show that syphilis is inoculable in the tertiary stage by means of vaccination? He did not press a reply, but thought the question was a good one for discussion at some future time.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A regular stated meeting of the Medical Society of the County of Kings was held on the evening of Tuesday, Sept. 21st, 1880, at Everett Hall, No. 398 Fulton Street. Dr. Charles Jewett, President, in the Chair, Dr. Nelson B.Sizer, Secretary *pro tem*.

The following named members signed the Register:

Dr. Charles Jewett, A. L. Lovell, N. B. Sizer, J. R. Vanderveer, V. Morse, B. Edson, P. H. Kretschmar, A. J. C. Skene, W. W. Reese, J. Walker, B. Ayers, W. H. Thayer, J. T. Wilbur, H. N. Read, G. F. Ketcham, W. A. DeLong, G. Wieber, H. B. White, B. S. Van Zile, T. M. Rochester, Thos. N. De Bowes, S. J. Sørensen, A. Matthewson, J. N. Freeman, J. Wilson, C. D. Cook, A. R. Matheson, J. H. Hunt, F. H. Stuart, H. F. Williams, E. J. Harvey, Wm. Wallace, J. A. McCorkle, Geo. Drury, A. J. Wohler, J. S. Prout, F. W. Wunderlich, S. Sherwell, W. H. Martin and Geo. K. Smith.

The meeting was called to order at 8.13 oclock.

The minutes of the previous meeting were read and approved.

The following propositions for membership were made:

Dr. J. F. Morgan, of Sheepshead Bay, L. I. Medical College Hospital, 1868; proposed by Dr. Geo. F. Ketcham; Dr. Anna M. Brown, of No. 488 Bedford Avenue, and Dr. Harriet F. Pillsbury, of No. 201 Rutledge Street, both graduates of the Woman's College of New York Infirmary (no date given); proposed by Dr. H. H. Pillsbury.

The Council reported the acceptance to membership of the following duly nominated candidates:

Geo. A. Schwartzenburg, M.D., University of Giessen, 1851; John Willard, M.D., University of New York, 1879; Geo. W. Wells, M.D., Bellevue Hospital Medical College, New York, 1868; Lucian T. Bell, M.D., L. I. Medical College Hospital, 1880.

The report of the Committee on Hygiene was then read by the Chairman, Dr. Walker (Dr. Sizer reading that portion of it relating to microscopical examinations), after making the usual acknowledgments of the labors of his associate members. The Chairman also stated that the Committee were sorry that the title as announced on the cards did not show the character of the paper to be presented. The exact title is "An investigation into the causes of alleged fatal and bad results following vaccination."

After the reading of the paper DR. HUNT said that inasmuch as the paper had not been properly announced upon the cards, and, therefore, members could not adequately prepare themselves for debate, he would move for a postponement of the discussion until another time.

The motion was seconded.

The resolution, therefore, being before the house,

DR. WALKER hoped that it would not prevail—as, in his experience, debates postponed never elicited that interest which always pertains to a debate immediately following a paper.

* The motion was lost, and the paper was discussed by Drs. H. B. White, H. N. Read, Stuart, Segur, Walker, G. K. Smith, Jewett, Burge, Williams and others.

On motion of DR. STUART, the resolution offered by the Committee was called up and adopted, namely:

"*Resolved*, That the Medical Society of the County of Kings, in view of the importance of pure and reliable vaccine virus being always attainable, do petition the National Board of Health to regulate and control the sale of vaccine virus."

On motion of DR. WALKER, it was also resolved that a copy of the resolution, together with a copy of the report, be sent to each member of the National Board of Health by the Secretary.

The second paper of the evening, entitled "Prophylaxis of Puerperal Convulsions," was then read by Dr. B. A. Segur.

It was discussed by the President as to the "uræmic" aspect of the etiology of Puerperal Convulsions; Dr. Martin, as to "the propriety of hastening delivery;" Dr. Skene, "the therapy of prophylaxis;" Dr. De Bowes, as to certain physiological aspects of the subject, and Dr. Stuart as to general considerations.

DR. DE BOWES moved that the discussion of the paper be continued at the next meeting of the Society.

DR. WALKER seconded the motion, on condition that the addresses be limited to ten minutes each.

DR. DE BOWES conceded the point.

DR. WALKER further asked whether the discussion would be the first order of business.

The PRESIDENT replied that that is usually understood.

The motion was then adopted.

DR. SMITH, as chairman, read the following obituary *in re* THOMAS BAYLIS, deceased, which was, on motion, received, and ordered to be spread upon the minutes:

THOMAS BAYLIS was born in Brooklyn, Sept. 1st, 1838. His education was begun in the excellent public schools of this city and supplemented by a thorough course under the tuition of Prof. Dwight in his school for boys. In early life his health was delicate, exhibiting a tendency to disease of the lungs, on account of which he spent several winters at the South.

While still quite young he manifested a strong desire to study the medical profession, taking every opportunity to learn by dissection, the structure of plants and animals. He spent two years in the drug store of Mr. Douglass, of this city, after which it was his rare good fortune to be a student in the office of that eminent surgeon, the late Dr. Charles E. Isaacs, of Brooklyn.

Under his instruction it was only natural that young Baylis should become proficient in the study of anatomy. Gifted by nature with a fine appreciation of art, he was able to sketch with his pencil and preserve for future reference the cases met in the clinics and the hospitals day by day.

Dr. Isaacs was not slow in making this talent serviceable, as many of the members of this Society will testify, who saw the beautiful drawings, made by Dr. Baylis, of the patient from whose brain Dr. Isaacs removed a large tumor.

After completing his term of study he graduated from the University of New York, in March, 1860, and commenced practice in Brooklyn. Soon after this event Dr. Isaacs died, and his loss was felt so keenly by Dr. Baylis that he resolved to retire from the profession, and engage in other business; for, as he said to the writer, "I had already made my arrangements to practice my profession with him."

In 1866 he was married to Miss Alice Hoyt, of Manchester, Vermont, by whom he had two children, one of whom and the widow survive him. After several years spent in business he at length returned to practice, and the Medical Staff of St. Peter's Hospital were prompt in securing his services as one of the Visiting Surgeons to that Institution.

He resigned this position in 1875.

Dr. Baylis was never a man of robust health, and during the last five years of his life suffered from repeated attacks of hæmoptysis. During the last year of his life it became evident to him that he was suffering from Bright's disease of the kidneys, and his death from asthenia occurred on the 15th day of June, 1880.

The following record of his long service in the Department of Health in this city is alike creditable to his fidelity and his skill :

"Dr. Baylis was appointed Assistant Sanitary Inspector in the Department of Health in May, 1873, and continued to discharge the duties of that office almost to the day of his death. Oftentimes when his medical friends advised him to remain at home on account of his physical weakness, he would insist upon attending to his daily work. His long connection with sanitary matters rendered his services very valuable to the Department, and his good sense and judgment will be greatly missed in that branch of the city government. His position brought him into constant relations with all classes of society, and with the result of making friends and no enemies. He was always genial and good-natured, no matter how vexing the circumstances might be in which he was placed. When his death was announced, his associates felt that they had lost one of the most trusted of advisers and one of the best of friends."

Abou Ben Adhem said to the Recording Angel :

"I pray thee, then,

Write me as one that loves his fellow-men."

So will we, in this last, sad parting, write the name of Thomas Baylis.

G. K. SMITH, M.D.,	} <i>Committee.</i>
J. H. RAYMOND, M.D.,	
S. E. FULLER, M.D.	

At this point the Secretary read the minutes of a Special Meeting of the Society in relation to the death of DR. THORNE, as follows:

Special Meeting of the Medical Society of the County of Kings was held at their rooms, on September 4th, Dr. Jewett presiding; Dr. Hunt, Acting Secretary.

THE PRESIDENT announced that the meeting had been called to take action in regard to the death of Dr. J. Sullivan Thorne.

Remarks on the life and character of Dr. Thorne were made by Drs. Otterson, Reese, Dudley and Mitchell.

DR. OTTERSON, as the late attending physician to Dr. Thorne, described his last illness and death.

On motion of DR. HUTCHINS, it was

Resolved, That the Society attend the funeral of Dr. Thorne, in a body, meeting for that purpose at the church, just previous to the funeral.

On motion of DR. OTTERSON, it was

Resolved, That a committee of four be appointed by the President to go to the house and accompany the remains to the church.

DRS. REESE, HALSEY, W. G. HUNT and J. H. HUNT were appointed as such committee.

DR. OTTERSON was appointed to notify the family as to the action which had been taken by the Society.

On motion, the Acting Secretary was ordered to print call for the Society to attend the funeral, in Sunday morning's *Eagle*.

On motion, adjourned.

J. H. HUNT, *Acting Secretary*.

On motion, the minutes of the Special Meeting were unanimously adopted.

THE PRESIDENT appointed as a committee to prepare a suitable obituary *in re* DR. THORNE, deceased: Dr. T. L. Mason, Chairman; Dr. J. S. Halsey and Dr. A. Otterson.

On motion of DR. WALKER, the sum of \$8.00 was voted for expenses of the Committee on Hygiene; and in this connection he remarked that it was the intention of the Committee to present one more report before the end of the year, and they will try to have an interesting one, and hoped that physicians to whom inquiries were addressed would respond promptly and fully.

The question proposed to be discussed is: "Why it is, if it is so, that Brooklyn is not a desirable place of residence for persons suffering from bronchial troubles?"

THE CHAIR declared that Dr. Joseph E. Smith had been duly elected an active member of the Society.

On motion, the Society adjourned.

Ἀσκληπιὸς



ὁ Σωτὴρ

Χάρμα μέγ' ἀνθρωποῖσι, κακῶν θελκτῆρ' ὀδυνῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—AS THE DISCUSSION on the prophylaxis of puerperal convulsions is to occupy the next meeting of this Society, the publication of the discussion at the last meeting has been deferred till the November issue of THE PROCEEDINGS, that the unity and continuity of the entire subject may be preserved. A number of short papers will be presented on special topics, and, in addition, the President desires a free, but brief, expression of experience on the part of members of this Society, on any of the questions raised in the original paper, and definitively, with the view of discovering to what extent medical men are accustomed to watch pregnant women with reference to the possibility of the recurrence of puerperal convulsions.

—SMALL-POX MORTALITY.—In Paris, in 1879, there were 858 deaths from small-pox; in the first four months of 1880 there were 1,038 deaths from small-pox. The legislature have now under consideration a law, already reported favorably, to make vaccination compulsory.

—CANNED FRUITS.—A recent case of poisoning by the salts of zinc, from eating preserved fruits, is curious from the fact that the fruit was a home-made preserve, put up in glass jar; the fault was in the fact that the cover had a metallic surface exposed to the acid contents of the jar, instead of having the customary porcelain lining.

—DR. FARR, of London, has received the title of C.B. from the British government. The *Lancet* regards this as an empty compliment as well as a tardy one, and neither better nor worse than the “hall-mark” with which sterling silver is stamped, but not beautified thereby.

—IRON AND ARSENIC is recommended by the *Chicago Medical Gazette* in menstrual irregularity due to neurasthenia, neuralgia and anæmia, as follows: Combine the tincture of iron, ten parts, with Fowler's solution, two parts. Dose, twelve drops after meals, in one-third glass of water, through a glass tube.

—*The Brain as an Organ of the Mind.* By H. Charlton Bastian, M.A., M.D., F.R.S. With one hundred and eighty-four illustrations. New York: D. Appleton & Co., 1, 3 and 5 Bond Street.

—*On the Bile, Jaundice and Bilious Diseases.* By J. Wickham Legg. New York: D. Appleton & Co., 1, 3 and 5 Bond Street. 1880.

—*A Practical Treatise on Tumors of the Mammary Gland; Embracing their Histology, Pathology, Diagnosis and Treatment.* By Samuel W. Gross, A.M., M.D. Illustrated by twenty-nine engravings. New York: D. Appleton & Co., 1, 3 and 5 Bond Street. 1880.

—*A New School Physiology.* By R. J. Dunglison, A.M., M.D. Illustrated with one hundred and seventeen engravings. Philadelphia: Porter & Coates.

—PORNOGRAFIA DE BUENOS AIRES.—De la necesidad imprescindible de un Dispensario de Salubridad y de una Oficina de Costumbres para reglamentar y reprimir la Prostitucion.

In a brochure with the above title Dr. Benjamin Dupont shows the necessity for a system which shall regulate and control prostitution. He believes it to be as much the duty of a municipality to repress it or to keep it within limits as it is to prevent the spread of contagious diseases and protect the lives and property of its citizens.

—THE PRESERVATIVE FLUID of the German government is made up of alum, 100 parts; salt, 25; nitre, 12; potass. carb., 60; arsenic, 10, and water, 1,000 parts. Mix, cool and filter. Then add to ten litres of this fluid four litres of glycerine and one of methylic alcohol. Two to five litres of this liquid will preserve a cadaver. Bodies saturated therein will, it is claimed, keep for years their form, color and flexibility. The method is partly by injection and partly by immersion. The smaller objects of natural history collections, as snakes, birds, fruits, butterflies, algæ, etc., are preserved as perfectly as larger bodies. Dr. Wickersheimer was the inventor of the process, the patent for which has been purchased by the government, and then by it the composition has been made public.

—AN EXCELLENT OPPORTUNITY is now offered for obtaining instruction in German. Dr. Deutsch, author of "Deutsch's Letters," well known as an eminent philologist, is forming classes for both beginners and those who already know something of the language.

The tuition fee will be placed very low, and the instructor's ability admits of no doubt.

We urge all who desire to profit by an exceptionally good opportunity to obtain further information by communicating with Dr. N. B. Sizer,

336 Greene Ave., who is prepared to answer all communications on the subject.

—SPIRILLUM FEVER, of Bombay, identical with relapsing, or famine fever, may be readily communicated to monkeys by inoculation with the blood of a fever patient. Dr. H. V. Carter, of Bombay, has tried this experiment upwards of a dozen times, with not more than two failures, inoculating from one animal to another producing characteristic general symptoms, and abundant parasites in the blood.

—HISTORY OF MEDICINE IN NEW JERSEY, AND OF ITS MEDICAL MEN, from the settlement of the Province to A. D. 1800. By Stephen Wickes, A.M., M.D. Newark, 1879. This is a handsomely printed volume of 450 pages, and is the most important contribution to the early history of American Medicine we have yet encountered. The author, who has the reputation of being one of the busiest practitioners of his locality, has found time to bring together a remarkable body of historical and biographical facts for the benefit of those who take an interest in the past of medicine. It has evidently been a labor of love, and its author is to be congratulated upon the successful issue of his volume. An appendix contains the verbatim reprint of a pamphlet, dated 1740, by Jonathan Dickinson, who was both clergyman and physician, "On that terrible Disease, Vulgarly called The Throat Distemper." This was a disease having all the symptoms of what is the diphtheria of the present day, the false membrane being called "a cankerous crust."

—EUONYMIN, a resinous substance derived from wahoo bark, has been brought into notice by the report of Dr. Rutherford to the British Medical Association, on the cholagogue action of drugs. The experiments of Dr. Rutherford with Euonymin appear to prove that it is a true cholagogue, or hepatic stimulant. It acts slowly, requiring about forty-eight hours, stimulates the intestines slightly, and does not occasion colicky pains. Unlike podophyllum, it does not affect the liver indirectly by catharsis. Hepatic dyspepsia, "biliousness," furred tongue, pale stools, *malaisé* and lassitude indicate its use. For the reason that its intestinal action is slight, it is best to combine it with compound rhubarb pill, or follow it by a saline laxative the next morning.

—TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COUNTY OF ALBANY. Vol. II., pp. 519, 1872. This volume covers the period from June 10th, 1851, to June 14th, 1870. It is, in many respects, a model record of the scientific and social doings of the profession of the locality named; it takes special pains, in its biographical department, to embalm the memory of its illustrious names, of which the number has been by no means small. Drs. Porter and James Bailey were the Publication Committee.

—CAUTION.—Physicians will do well to be on their guard against the operations of a plausible canvasser of an illustrated serial. Any such person representing himself as introduced or recommended by prominent physicians should be required to show some written credentials.

—LITERATURE AND MEDICINE is the title of an article in the July *Eclectic*. It treats of the various points at which the two professions touch. Among others is the kind tendance given to men of genius by those whose care and duty it is “to stand between man and his doom.” “Who can forget Dryden’s grateful acknowledgment of the services of Hobbes and Guibbons? or Cheselden’s goodness to Pope? or Meade’s to Gay? or Arbuthnot’s to every literary man with whom he came in contact? ‘There is no end of my kind treatment from the faculty,’ writes Pope, a few weeks before he died; ‘they are in general the most amiable companions and the best friends, as well as the most learned men I know.’”

—WE were lately shown a specimen of six-inch iron soil-pipe taken from a tenement house in Brooklyn, where it had been in use some twenty-two years, at first connecting with a privy, and latterly with the sewer. The soil-pipe was 70 feet long and had no ventilation excepting from a 4-inch leader. There were three families in the house. The drain was doubtless only partly filled most of the time, hence all the upper portion was corroded to a most extraordinary degree, the iron having oxidized in many places until it was not thicker than a book-cover, and it could easily be broken with a slight blow, and even with the fingers. A better illustration of the consequences of lack of ventilation could hardly be afforded.—*The Plumber*.

—EFFECTS OF SMOKING ON THE HEART.—Some years ago M. Decaisne drew attention to the fact that tobacco-smoking often causes an intermittent pulse. Out of eighty-one great smokers examined, twenty-three presented an intermittent pulse, independent of any cardiac lesion. This intermittency disappeared when the habit of smoking was abandoned. He also studied the effects of smoking on children from nine to fifteen years of age, and found that it undoubtedly caused palpitation, intermittent pulse, and chloroanæmia. The children, furthermore, became dull, lazy, and predisposed to the use of alcoholic drinks. Recently he reported to the *Société d’hygiène* the results of his observations on the effects of smoking on women. Since 1865 he has met with and observed forty-three female smokers. Most of them suffered from disturbances of menstruation and digestion, and eight presented very marked intermittency of the pulse without any lesion of the heart. He gave detailed accounts of these eight cases, in which all treatment directed against the intermittency proved utterly useless, while the suppression of tobacco was invariably followed by improvement, and very often by complete disappearance of the phenomenon.—*Gazette Obstétricale*.

SEE PAGE 294.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The October meeting will be held on the 19th, at which there will be presented the following:

Adjourned Discussion of Prophylaxis of Puerperal Convulsions.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

President..... C. JEWETT, M.D., 307 Gates Ave.
Vice-President..... G. W. BAKER, M.D., 48 Bedford Ave.
Secretary..... R. M. WYCKOFF, M.D., 532 Clinton Ave.
Assistant-Secretary..... J. H. HUNT, M.D., 419 Hart St.
Treasurer..... J. R. VANDERVEER, M.D., 301 Carlton Ave.
Librarian..... A. HUTCHINS, M.D., 796 De Kalb Ave.

CENSORS.

B. A. Segur, M.D., 281 Henry St. F. W. Rockwell, M.D., 6 Lafayette Ave.
 E. R. Squibb, M.D., 36 Doughty St. L. S. Pilcher, M.D., 4 Monroe St.
 J. D. Rushmore, M.D., 129 Montague St.

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK.

(1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI., Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker, B. Edson, N. B. Sizer.

REGISTRATION.

Drs. W. E. Griffiths, Drs. W. G. Russell, Dr. R. M. Buell
 A. S. Clarke, N. Matson,
 J. A. Jenkins, F. W. Rockwell.

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur, W. W. Reese, J. H. H. Burge, A. Hutchins, W. G. Russell.
 I. H. Barber, A. W. Catlin,

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

NOVEMBER, 1880.

No. 9.

THE PROPHYLAXIS OF PUERPERAL CONVULSIONS.

(See Dr. Segur's paper in October number.)

DISCUSSION.

DR. C. JEWETT, in opening the discussion, remarked that eight cases of puerperal convulsions had fallen under his observation. One occurred in his own practice, the others in the practice of other physicians.

CASE 1.—His own case—seven and a half months pregnant; urine strongly albuminous; sp. gr. 1010; seized with convulsions at onset of labor before treatment could be fairly established; recovery.

CASE 2.—Anasarca and nausea during last few weeks of gestation. Protracted labor; forceps delivery under ether; first convulsion one hour after delivery; death on third day, of pulmonary œdema.

CASE 3.—Seven months pregnant; marked œdema for weeks prior to attack; vomiting and diarrhea on day of seizure; death.

CASE 4.—General œdema; delivery during convulsions; death.

CASE 5.—Slight œdema of lower extremities; cephalalgia for ten days prior to seizure; delivered two days after first convulsion; death.

CASE 6.—Eight months pregnant; anasarca and headache for weeks; rapid delivery by aid of Molesworth's dilator and forceps after third convulsion; recovery.

CASE 7.—Notes incomplete. First convulsion after delivery of twins; recovery.

CASE 8.—Diagnosis of chronic renal disease; recovery from convulsions; died, three months after delivery, of rapid phthisis.

In all these cases, except one of those in which the history is incomplete, there is evidence of renal disease prior to the seizure. They were all of them, in his judgment, uræmic. Basing his opinion, therefore, on his own experience, he was inclined to the belief that puerperal convulsions of non-uræmic origin are the rare exception. He could concede, of course, that other factors may co-operate in the causation of convulsions either directly or indirectly; such as hydræmia—the excitable condition of the nervous system that obtains during pregnancy and labor, or the reflex irritation of the process of labor. That the convulsions in so large a proportion of cases occur during the labor, lends some color to the reflex theory. That reflex irritation is not a very prominent factor, is rendered probable by the fact that so many cases occur before the labor begins or after its completion.

Dr. W. H. MARTIN: As evidence that wide differences of opinion still exist, and that, therefore, such a discussion as this is not unnecessary, I will make a few brief quotations: Professor Carl Schroeder, in his *Manual of Obstetrics*, in his chapter on the treatment of puerperal convulsions, after advising the use of venesection, chloroform, and especially morphine, goes on to say: "No other treatment, and especially no kind of obstetric manipulation, is required for the safety of the mother. The condition of the woman contra-indicates obstetric manipulation, and every irritation of the uterus, such as the introduction of the hand for the purpose of turning, the application of the forceps, or even mere friction of the fundus, is apt to cause a fresh attack by reflex action."

Let us quote in contrast what was said by Dr. S. G. Thomas, of N. Y., in the course of discussion upon a paper by Dr. Richardson, which appears in the *Transactions of the Gynecological Society*, Vol. III., 1878. After noting slight cases of albuminuria, which do not demand interference, Dr. Thomas says: "In cases where the amount of urine is small, where it is rendered more than half solid by nitric acid, and where there is stertorous breathing at night and marked œdema, I believe nothing except premature delivery will, as a rule, prevent puerperal convulsions." In the same discussion Dr. B. F. Barker, of N. Y., stated his belief that "where there are symptoms which severely threaten the life of the mother or child, premature labor should be induced."

By one authority, therefore, we are enjoined not to interfere even when labor has commenced and convulsions have occurred; by others we are advised to excite premature labor to prevent convulsions. It is difficult to reconcile these widely diverse opinions of others, and we will only endeavor to give the reasons for our own.

It will facilitate discussion to divide puerperal convulsions into two classes: I. Those which occur some time before the completion of gestation; II. Those which occur at full term.

I. When pregnancy induces that assemblage of conditions which is called albuminuria, very threatening symptoms may occur early in gestation. I reported a case to this society, some years ago, in which, at the third month, the woman's condition was so threatening that it was considered necessary to produce abortion. Here there was no Bright's disease, the symptoms all disappeared as soon as the foetus was removed, and the woman is now perfectly well. Such cases, however, are rare, and convulsions seldom occur before the seventh month, except in chronic Bright's disease, where, it may be remarked, abortion is very apt to precede or accompany the approach of the first convulsion. To save time, therefore, and also because operative interference is rarely needed before that period, I will consider only those cases which occur at or after

the seventh month of pregnancy. The question, then, is: Are we to induce labor at this period to prevent convulsions which are imminent?

The propriety of any operative procedure is established whenever its advocates are able to prove: (1.) by recorded experiences that it is *necessary*; (2.) by practical example that it is *feasible*; (3.) by exhibition of results, that it is *safe*. Let us apply these tests to the induction of labor, in these cases:

1. As to its necessity, it is sufficient to show that it can accomplish the end proposed when other means have failed, or are very likely to fail. Now the remote cause of puerperal convulsions is, axiomatically, the presence of the child in the uterus. Pregnancy is, in an overwhelming proportion of cases, the sole and only excitant of those pathological conditions which culminate in convulsions. Pregnancy is the cause of convulsions; remove the cause, the effects disappear. Theoretically this is *always* true, practically, it is often true. When the ending of pregnancy does not prevent convulsions, it is because pregnancy has not ended soon enough to prevent the pathological state it has induced, from doing permanent damage to the blood, or the central nervous system, or the kidney. And this, of course, is the explanation of post-partum convulsions. It is a fair inference, however, that if the cessation of pregnancy will not destroy, it must impair the activity of the abnormal agencies that pregnancy has created. But however firmly we may believe this, we are certainly not to interfere in every case where threatening symptoms develop during pregnancy. Dr. Thomas is on record as declaring that the man who believes that labor must be induced in every—even severe—case of albuminuria, is a most dangerous man. And all practical observers will agree with him. Cases are of every-day occurrence where, under proper treatment, the most threatening symptoms disappear, and the rest of the pregnancy and the parturition are perfectly natural and safe. What, then, is to determine the necessity for interference?

All cases likely to occur may be divided into two classes: *a.* Those in which the very first indication of trouble is a convulsion or unmistakable symptoms of the approach of one. Here, most certainly, no attempt at inducing labor should be made, until general remedies have been actively and persistently employed; and even if these prove unavailing, interference should be resorted to more in the interest of the child than of the mother. If the symptoms *do* yield to treatment, and yet after some days the intelligence remains clouded, and especially if only a small quantity of highly albuminous urine is secreted, evacuation of the uterus affords the only hope of saving the woman's life. *b.* The other class of cases is that where albuminuria has been recognized early and the woman has been put under careful and thorough treatment, with only partial success. That is, if, although all apparent cerebral congestion, although the œdema, the headache, the disturbances of vision have disappeared—if, in spite of this improvement, the urine remains after weeks of treatment very scanty and full of albumen—I maintain that we must induce labor to prevent convulsions, and that we will be most likely to succeed if we interfere early enough. I need scarcely say that when *all* the symptoms improve, and the child is still alive, the pregnancy should be allowed to reach full term, and labor to commence without interference.

2. As to feasibility in the sense of being possible of accomplishment, I believe that with the means now at our command, no os nor cervix will resist proper and persistent efforts at dilatation. In the sense of its being possible to accomplish sufficient dilatation for delivery *in time to use it as prophylactic* of convulsions, interference in any individual case must rest on the experienced judgment of the attendants. The decision should be governed by the condition of the child as well as that of the mother. Every effort should be made to save a living and viable child, especially if the mother's con-

dition be progressively bad. If the child is not viable, only the most pressing danger of the mother should make us resort to its extraction. If the child is dead, the mother will be subjected to little more danger by its removal, than in the already depraved condition of her blood she will be exposed to by carrying a decomposing mass in her womb.

This brings us to, and partly anticipates the third question, that of safety to mother and child. The only danger that I deem it necessary to consider in this connection, is that supposed to threaten the mother, exciting reflex action by the manipulation necessary to induce labor. If the views of Schroeder, already quoted, were undeniably and invariably true, this danger would be worthy of grave consideration. But I believe that the danger of exciting convulsions by reflex action, especially in uræmia, is exaggerated. I have myself, in several cases, used nearly every known means to dilate a rigid cervix, and have only once had any reason to believe that an ensuing convulsion was the consequence of the manipulation. But even if reflex irritation is a constant factor in the exciting of any one convulsion, it is surely better to arouse a single attack in the probably successful effort to prevent the occurrence and continuance of many others, than it is to do nothing in the face of an absolute certainty that they will come if delivery is not effected. To refuse to interfere for this reason alone, is about as sensible as it would be to refuse to give a hypodermic injection in tetanus, because the irritation of the needle might excite a paroxysm.

II. We come now to the obstetric management of labors at full term, where convulsions are imminent. The *propriety* of interference is much more manifest here than it is in cases occurring early in pregnancy; but the indications for interference are about the same. As to the propriety of interference, I think the ground is sufficiently covered by what has been said already. As to the indications for interference, I will state them in the form of propositions, which represent my own belief and govern my own practice.

1. Whenever threatening symptoms appear in a case that has been observed and treated, an examination should be made, and if the os is undilated and rigid, the douche should be used, or if dilated or dilatable, other means to hasten delivery should be at once employed.

2. Whenever, as happens in a great majority of cases, the immediate prodromata of a convulsion are the first-observed evidence of mischief, general remedies should be actively used before even examining the condition of the uterus. If these remedies are effective, labor will, in all probability, begin, and the rapidity with which it progresses should decide the question of interference.

3. If the general condition improves, and no natural efforts at delivery occur, no interference should be attempted so long as the condition remains good.

4. If, in spite of treatment, the symptoms grow more threatening, if convulsions occur and continue, the os must be dilated and delivery effected just as rapidly as is consistent with the safety of the mother and child.

5. If convulsions have occurred and labor has begun, no unnecessary delay should be permitted, in any case.

We will now consider briefly, "the means for hastening delivery."

To hasten the first stage, we have to begin with the uterine douche. The great value of this consists largely in its being available, and generally successful when nothing else can be done; that is when the os is absolutely closed. I will speak of it further only to make the practical point, that the stream should be not intermittent, but constant, and after dilatation has begun, should be persistently directed against one point of the circumference of the os.

Next in order come sponge tents, the usefulness of which is almost limited to cases occurring before full term, in which the whole cervix, as well as the os, has to be dilated.

Next, we may employ dilators, using air or hydrostatic pressure. Of many different kinds, the one I would recommend is that invented by Dr. A. L. Lowell, of this city. It is easily introduced, not liable to get out of position, and very durable. It is also capable of being, while in situ, a medium for applying either heat or cold.

Next, we may use manual dilatation, a measure which, it seems to me, is much underrated. Besides being itself useful in many cases, it is our best source of information as to how much we may expect from other modes of treatment.

Incision has been resorted to when other means have failed. It seems to me that there is but one set of circumstances in which it is permissible, namely: where the mother's condition is hopeless and the child is alive, our only alternative in this situation being the Cæsarian section.

Finally, as a means of hastening the first stage of labor, we have the forceps—rarely used for this purpose, but most efficient. At a meeting of the Dublin Obstetrical Society, held in November, 1878, Dr. Geo. Johnson, ex-Master of the Lying-in-Hospital, made a report of 752 forceps deliveries, in a total of 7,862 labors. In 169 cases forceps were applied before the os was dilated; in 59 of the 169 the os-uteri was only two-fifths dilated. He maintains that in none of these cases was the os-uteri more fissured than is usual in first deliveries. The reason for interference in 42 cases was early rupture of the membranes.

Dr. I. E. Taylor, of New York, claims to have used the forceps successfully through an os measuring seven-eighths of an inch, in two cases; and in a more recent paper he strongly advocates the use of forceps in the first stage to prevent possible exhaustion from protracted labor. This reference to others emboldens me to say that I have used my own forceps (a modification of Elliot's, with a blade one and three-quarter inches wide), through an unabsorbed cervix at the eighth month, which was by measurement an inch and a half in diameter. In this case delivery was easy and comparatively rapid. But the os was dilatable, and here lies the gist of the whole matter. The forceps will dilate a dilatable os better than any other means I know of. The increasing size of the wedge as traction is made maintains a regular increasing pressure, which is almost certain to be effective. When the os is *not* dilatable it is quite a different matter. The forceps may be carefully used, however, for a few minutes at a time, and their employment may be alternated with other means of dilatation.

As to means for hastening the second stage, I need only say that they comprise every operation known to obstetric art; and that the rules for their selection and employment are those which govern in any case of delayed or obstructed delivery. But in cases of puerperal convulsion it is well to remember that, "If it were done when 'tis done, then 'twere well it were done quickly."

DR. F. H. STUART: The very idea of prophylaxis carries with it the consideration of the *causes* of puerperal convulsions. It implies a *knowledge* of the causes. We cannot prepare for or ward off an enemy whose point of attack, mode of warfare, or strength we do not know. It implies *foresight* in contradistinction from *after-sight*.

Let me dwell upon the word prophylaxis. What does it imply? It implies the ability to diagnose the cases in which convulsions will occur if not interfered with. We speak of the prophylaxis of typhoid fever, or of small-pox, or of scarlatina, or of any other disease—even of the chronic diseases, as of phthisis. We consider in an

analytical way the causes that produce these diseases, and then endeavor to ward them off.

When we seek for causes of other diseases we usually go to the dead-house and examine the body post-mortem. For instance, a patient dying from typhoid fever presents such and such lessons, had such and such hygienic surroundings, and we say the case is perfectly clear. There is some uniformity in the history of successive cases. This same method of reasoning is too often, indeed is generally applied to cases of puerperal convulsions. Statistics of cases in which convulsions occurred are compiled in this manner. The cases are not classified in a broad and philosophical way and with an unbiased mind. Most frequently this is done by some one who has a pet theory to establish or maintain. There will be brought together a set of cases, every one of which had uræmia, and, *post hoc ergo propter hoc*, uræmia was the cause of the convulsions! There is another set of cases, collected from another standpoint, where the urine examined previous to labor showed no albumen or evidence of retention of urea and uric acid in the blood, but there were other conditions present, as plethora, chlorosis, anæmia, and so convulsions are explained as due to these causes. Hence, in the consideration of the subject before us, one will say uræmia is the cause, and we must eliminate that, another will say that the cause is the condition, which Cazeaux says always exists in the pregnant woman (though few, I think, will agree with him), of anæmia and chlorosis, and so this must be counteracted. Again, another finds the cause to consist in a condition of "nervous irritability," which obtains in pregnancy. With this one the bromides will be a favorite remedy.

The question then arises, *Can we have a prophylaxis?* In many cases where we expect convulsions they do not occur. Prof. Tusk, of New York, in October, 1873, published a lecture in which he mentions 82 cases of Bright's Disease, only two of which had convulsions. Every one of us have examined the urine of our patients, and finding albumen and hyaline casts, have awaited the summons to those patients with mortal terror; and yet how many are thus watched with the greatest care who do not have convulsions! I must not dwell upon this point. We cannot reason in this way. Patients whom we expect will have convulsions do not have them, and others who show "premonitory" symptoms—as we so often call them—go through labor and the puerperal state without a convulsive moment.

Let us consider for a moment what a convulsion is. We may say that it is a tonic, clonic contraction of muscle. If we have a colic we have a convulsion of the intestine. How much does this differ from the physiological contraction of the intestine? Where is the line of distinction? If the uterus contracts to expel its contents, there is a condition of convulsion. Now, what we call puerperal convulsions is this condition extended to a larger number of muscles, acting by reason of some irritation of the nervous system. *The function of the nervous system is to respond, in its normal condition, to a moderate stimulus.* That is the condition of the nervous system in health. The nervous system, like all the other systems or organs of the body, is dependent for the normal performance of its functions upon a regular and sufficient supply of pure blood. Hence, the factors in a healthy action of the nervous system are: (1) a due supply of healthy blood, and (2) not an unnatural degree of stimulation or irritation.

In pregnancy there is, physiologically, an *unusual* susceptibility of the nervous system to *unnatural* disturbances or *excessive* irritation. This constitutes the predisposing condition of puerperal convulsions. No doubt there exists in very many cases, if not in all, the disturbed circulation, and the deteriorated quality of the blood. But the nervous system of the individual, being less susceptible, wards off the attack.

This leads me to offer a possible explanation of the wide discrepancy in the statistics

of convulsions. Those gathered from private practice are among a class of patients whose nervous system, habitually overwrought by their social surroundings and habits of life, show a much larger proportion of cases of convulsions than do those made up from hospital practice. In 51,000 cases of delivery at the Rotunda Hospital in Dublin, during a period of 83 years, there were only 138 cases, or about one in 360 confinements. In this latter class (hospital patients, and those of that social condition) the muscular system is better developed. They live more natural lives. And (a fact of great significance) they do not receive the attention from the doctor, nor do they know enough of the dangers of pregnancy to have their fears aroused. They approach labor with less anxiety and more of hope of a safe confinement. Let me ask: Do we ever, by our frequent examinations of urine, and in other solicitude for our patients, so disturb their peace of mind, and hence their general health, that they have convulsions when they else would not have done so?

Another fact deserves notice. If I remember correctly, Dr. Segur, in his paper, stated that among the French convulsions occurred more frequently than among the English or Germans. This is a very significant fact when we consider the general national temperaments and characteristics in these different people. When I mention the Frenchman you think of a nervous, highly-strung organization; and a German cool, philosophic, stolid; while the English are more vigorous—the muscular system being developed by following the excellent rule of frequent and regular exercises. In these general facts (for specific rules cannot be relied upon) we are able to discover some leading indications, which will guide us in the prophylaxis of puerperal convulsions.

I think we cannot give specific directions in regard to prophylaxis. It is rather a question of physiology. We can attend to the general health, regulate the excretory organs, quiet the nervous system as much by our own quiet hope and confidence as by the administration of bromide or chloral. There is no specific remedy either to prevent or cure convulsions. But I apprehend that great good will result from the presentation of this question of prophylaxis, in that we shall study our cases more carefully—considering the surroundings, the temperament and the constitutional peculiarities of our patients, so that if they do have convulsions we shall be ready for the onset in that we shall not indiscriminately apply the lancet, or chloroform, or morphine or any other remedy which, being inappropriate to that *particular* case, will lay the burden of our pernicious treatment and ill-advised interference upon our helpless patients. But we will, when all others are panic-stricken at the terrible struggle, as coolly and calmly consider the case as we do any other to which we are called.

NOTE.—In regard to Dr. Segur's calculation of the probable number of cases in this city, I will say that I think there is a mistake. It seems so very improbable.

DR. A. J. C. SKENE had been told that in the process of mental evolution, the condition which immediately precedes knowledge is a well-defined consciousness of ignorance; and in view of that fact he hoped some day to know something of puerperal eclampsia. Were he to be guided just now by what he knew as compared with what he did not know, he should decline to say anything on the subject. But as he understood the intention of the present effort was to bring out, as far as discussion can do so, some new fact, and therefore lead us to a clearer comprehension of the whole subject than prevailed before, he would, therefore, endeavor, as briefly as possible, to perform his part in the discussion; for it is incumbent upon one to gather together all that he can of the facts of this subject, and arrange them in his mind, so as to have them ready for use on any and all occasions, when he may be called upon to manage such cases. He

had done this in the past, and, therefore, would be satisfied to briefly state what his views are with reference to this question of prophylaxis. But, in order to do so in an intelligent and proper manner, he would be obliged to state what he has always believed to be the cause of puerperal convulsions, because he could not hope to be clearly understood in reference to the subject otherwise.

First of all, he believed that there is a constitutional condition predisposing in individual cases to puerperal convulsions. He believed this from the cases which he has seen, because there has been something of a uniformity in the cases, and all of them have been of a peculiar organization, approaching the chlorotic condition, as it is called—that is to say, women in whom, in proportion to the other organs of the body, the heart and blood-vessels were small, and the respiratory apparatus was also comparatively small, and in consequence of these conditions there is a tendency to an adipose development, together with a peculiar characteristic condition which falls short of well-marked chlorosis. That peculiar kind of an organization stands as a predisposing cause of puerperal convulsions.

Then again, pregnancy produces great changes in the organization. Barnes formulates that thought very well where he said, “There is no physiological condition which so nearly approaches pathology as that of utero-gestation.” We know some of these changes very well. There are many modifications of the nervous system caused by pregnancy, and it would be exceedingly interesting at this moment if we could take up the discussion of the relations of the generative organs to the brain and nervous system during pregnancy. We would then be able to see more clearly the predisposing conditions of puerperal convulsions as they relate to the nervous system. That the nervous system is influenced we know by the reflex phenomena manifested during utero-gestation. There must be an increase in the excitability or an increase in the quantity of the organic system, or, perhaps, both, as a necessity, in order to accomplish the increased nutrition. We know that there is an increase in the quantity of muscular tissue in the heart, the hypertrophy of pregnancy, and it is reasonable to believe that a like hypertrophy of the organic nerves occurs at the same time to meet a similar demand in the increased nutritive process. Then the law comes in that in all cases where a given portion of the organs is highly developed and at the same time takes a maximum growth, we have a predisposition to acute forms of disease. Another change is in regard to the circulation. There are alterations in the dynamic condition of the circulation, and there is a change in the condition of the blood itself. There is a diminution of the blood globules and an increase of the serum, a hydræmic state of the blood. There is a change in the distribution of the blood in the body. There is early established, and maintained during the later months of pregnancy, a venous hyperæmia, especially in the lower parts of the body; a condition of the organization and circulation of the blood which reacts upon the urinary organs and predisposes to derangement of functions or the eliminating office of the kidneys. Here then we have a changed condition of the nervous system, and of the circulatory system, which predisposes to functional disease of the kidneys.

While there is a venous hyperæmia of the lower portion of the body, there is also an arterial hyperæmia of the upper. This arterial congestion produces increased tension of the nerve centres, and thereby favors nervous derangements. This increased excitability of the surcharged nerve centres intensifies reflex action upon other organs of the body, thereby predisposes to puerperal convulsions. The fact is that at the present day almost everything abnormal in the human body is charged to some nerve disturbance; and we are told by one author, De Costa, that Bright’s disease can be traced to a derangement of the nervous system. However, there is, as we have seen, a de-

rangement of the nervous system and a derangement of the circulatory system, which predisposes to a derangement of the urinary system. But in drawing our conclusions as to the exact part which one or the other system plays in the causation, we cannot say that the circulatory or the nervous system predominates in this regard; we have to take the whole into consideration in estimating the causes which give rise to a predisposition to puerperal convulsions. Primarily, then, we have a condition which predisposes to this disturbance of the functions of the kidneys; and, secondly, an uræmia, which acts as a most important, if not *the only* exciting cause of convulsions. We have a mental element also; but the particular point is the uræmia. There is another point which has been referred to in the paper of this evening, and that is the presence of albumen in the urine. The fact that albumen is not present during convulsions, or before convulsions, is no proof that the kidneys are active, normally. It only proves that albumen is not there, and should not be taken as evidence that the kidneys are performing their function perfectly. I believe that blood-poisoning may, to a limited extent, arise from imperfect elimination by the kidneys while there is no albumen in the urine. This is especially likely to occur when the kidneys are deranged in function and not changed in structure. It is as easy to conceive that the kidneys might fail to eliminate urea under the deranged nerve influences of pregnancy as to accept the fact that the stomach would refuse food because of reflex disturbance. It should also be borne in mind that an amount of uræmic poisoning which would cause no great disturbance of the nervous system in ordinary conditions would be sufficient to cause convulsions in one whose nervous system was modified by pregnancy.

In view, then, of all these facts, the speaker thought there is but little doubt that the exciting cause, in a large majority of cases of convulsions, is this uræmic condition. At any rate we have no facts to prove the contrary. We have no well-recorded class of cases of puerperal convulsions in which the fact has been demonstrated, that the kidneys are fully doing their duty.

Now, then, if he had made himself clear with regard to the causes of eclampsia (and he had only hinted at the points), then this question of prophylaxis will be much more easily dealt with; and because of the lateness of the hour, he would discuss this portion of his subject briefly.

So far as prophylaxis is concerned, he meant to limit himself to that which may be done in the management of cases previous to the appearance of convulsions. Can we guard the patient against puerperal convulsions by removing the causes which predispose to and excite them? That really embraces the whole subject of the management of patients during the puerperal state; it takes up the whole subject of the hygiene of the pregnancy. The questions of diet, of clothing, of exercise, are important. The food used in pregnancy should be carefully adjusted to the wants of each case. In general, nutritious, easily-digested, non-stimulating diet is all that is required. We know that in the treatment of Bright's disease highly nitrogenized food is objectionable, and, therefore, milk has become the favorite article in the diet of those suffering from albuminuria. From this we may infer that a limited amount of animal food should be used by pregnant women. It is also well known that primiparæ are usually possessed of phosphates in abundance. This is shown in the superior development of the bones of first-born children. In those who are less or more exhausted by frequent child-bearing and lactation, there is an unusual demand for phosphates. These facts should guide us in prescribing food and medicines for pregnant women. In nervous exhaustion phosphates are called for, and in abnormal excitability they may be withheld with advantage.

By careful attention to clothing and surface temperature a very important point would

be gained in keeping the skin in a normal condition, so as to give the kidneys less to do. We must simply require of them what belongs to them normally, and guard them from over taxation. When you know the tendency to imperfect elimination, and the dangers which follow, means can be used to overcome the one and prevent the other. He thought it was well that this should always be attended to, whether we have albuminuria or not. Then, again, a word as to other methods to be employed to secure free elimination. Constipation should be overcome by the employment of laxatives. Salines are employed with advantage when there is hydræmia. With salines it will be well to use tonic treatment. Sulphate of magnesia and sulphate of iron answer well. Salines produce watery stools and relieve the uræmia; and Dr. Mitchell used the bicarbonate of potash for the same purpose with excellent results. These salines may be given to overcome constipation, even if there is no kidney trouble, for it is better to anticipate than to wait until the uræmia occurs before doing anything. By keeping the bowels freely in action you help the liver to perform its duty also, and by so doing you relieve the kidney. He had an idea that torpidity of the liver had considerable to do with this question of imperfect elimination. It is well, when there is torpor of the liver as well as constipation, to give mercurial cathartics. He believed that many of the benefits derived from mercurials came from their action upon the kidneys, for he believed that mercury is a remedy which stimulates the kidneys to higher action. It is necessary also to modify the cathartic carefully, so as not to disturb the process of gestation; but in order to produce abortion a more active cathartic is required than any one would give for the object in view. He never hesitated to give cathartics unless there is a tendency to abortion.

Another indication to be fulfilled is equalization of the circulation. Now, you know, notwithstanding some authors to the contrary, that the pregnant condition demands a largeshare of vital force, and therefore the patient requires, to a certain extent, a very general development, which can only be secured by judicious exercise. Certain positions of the patient during this state very greatly tend to increase or diminish the amount of blood in given portions of the body; as for instance, *sitting* in a chair prevents the blood from a free return circulation from the lower portions of the body and extremities. He had always insisted upon the point that nothing has so much power in equalizing the circulation as muscular exercise; and he is perfectly certain that no other influence can be brought to bear that will so counteract evil tendency arising from the reproductive organs as EXERCISE. He therefore urges always that patients should take a large amount of muscular exercise while in the pregnant condition, and then to rest frequently in the recumbent position. Exercise and rest, systematically followed, will take the pressure off the renal circulation and relieve this natural tendency to engorgement better than any known remedy. Then again, exercise helps to maintain the nutritive system in its best possible condition.

The management of the nervous system requires marked attention. There is a heightened functional action which often gives rise to irritability, capriciousness, sleeplessness and all those nervous disturbances so often present in the pregnant state. For these we should employ nervous sedatives, and among them, none are so beneficial as the bromides. Among the advantages of these remedies, it may be said that they leave no evil effects upon the pregnant system, and they are often of the greatest benefit in overcoming all such nervous disorders. The speaker remembered that years ago Prof. Barker, of New York, published a paper on the influence of belladonna in shortening the first stage of labor; he stated it as a fact that in patients kept under its influence during pregnancy the first stage of labor was shortened. I have an impression also that he has recommended it as a means of preventing kidney complications and con-

sequently of modifying puerperal convulsions. Dr. Skene thought there was something in that. How much it influenced the dilatibility of the cervix he did not know; but that it had a beneficial influence upon the nervous system, he felt convinced; and whenever he had found a patient with a severe headache with sleeplessness and kindred symptoms, he has used belladonna when the bromides did not answer with benefit.

Now, apart from these two drugs, he had but little well-grounded experience. He had heard that chloral in small doses was of benefit. In this connection it may be well to mention that popular remedy, massage. The judicious use of it fulfills to a certain extent all the indications referred to, except, perhaps, elimination. It takes the place of muscular exercise, helps to equalize the circulation, and is a great nerve sedative when agreeably employed. He had employed massage and had seen benefit from it. Whenever the nervous system shows the slightest deviation from health the nervous sedatives should be employed. He believed that if the nervous system were kept in a quiet, unexcited state during the pregnant condition, better results would follow than otherwise; and this in a degree explains why it is that primiparæ are more liable to convulsions than multiparæ. It is because of the state of the nervous system mental disturbances are far greater in the primiparæ than in others. They are naturally very much exercised over the probable result of the great trial which they are about to undergo; and if they can be kept as to their nervous system and circulation on the normal plane, he believed the most important points in the prophylaxis of puerperal convulsions would be attained.

DR. A. HUTCHINS: The topic assigned to me is the non-uræmic puerperal convulsion. With any recognized use of terms I do not believe in its existence. Of course, it would be arguing in a circle to define puerperal convulsions as convulsions occurring in the pregnant or parturient woman dependent upon, or associated with, uræmic poisoning, and then to assert that puerperal convulsions cannot occur independent of the uræmia. That the epileptic seizure, the apoplectic fit, the hysteric attack, and other forms of tonic and clonic spasm, may occur in the course of pregnancy is accepted, but no one of these would be confounded with the form of convulsions which are the crowning horror of the lying-in-room, which are, unfortunately, familiar to all, and which have a literature recognizing them as an accessory phenomenon to uræmia. Preference may be given to a form of statement that they are an "accessory phenomenon to any disease capable of producing sufficient uræmic poisoning to irritate the medulla." Preference may be given, likewise, to any other form of statement; but any proposition that ignores the uræmia throws down the gauntlet to the present state of knowledge on the subject. So, while all are in a receptive mood, because of the obscurity of the question involved, on the questioner must rest the burden of proof.

This being true, and the subject of prophylaxis being the salient point of this discussion, it is of surpassing moment to know whether the conditions that precede the occurrence of puerperal convulsion are, in all cases, recognizable in advance; whether, if sufficient care be exercised, these conditions can be perceived, giving grounds for instituting treatment that may prevent the convulsion. Is there, or is there not, an unannounced puerperal convulsion?

Uræmia in pregnancy is diagnosed by the same symptoms and by the same tests as when occurring independent of the pregnant condition. There can be no doubt about these, and it is not essential to the present inquiry to point out how the hyperæsthesia of the pregnant woman, or her pathologic state, if you please, facilitates the onrush of

the convulsions, when any extent of uræmia exists. Convulsions do not occur in all recognized cases of acute or chronic uræmia, whether the uterus be gravid or whether there be no uterus to be gravid. However numerous the chances in favor of their occurrence, still they do not always follow.

Over against this put the other fact that when the convulsion has come unexpectedly, and an examination invariably demonstrates the existing albuminuria, while the convulsion did come unexpectedly, is the accoucheur to be burdened by the reflection that there was a pre-existing uræmia which was an ascertainable fact?

Personal experience is asked for in this discussion. I have none to offer; nor do I know of any recorded observations, worthy of quotation, bearing directly on this point and at all conclusive for direct argument. I hold the affirmative of this question, and yet it must be acknowledged that, up to this present, the argument is wholly inferential. If a pregnant woman should be known to have arrived at term absolutely free from any of the recognized objective symptoms of uræmia; if skillfully applied tests have demonstrated the absence of albumen in the urine, and a proper excretion of urea, and then, at the onset of labor, convulsions ensue, and albumen be found in the urine, as it always is found, there would be a demonstration of the unannounced puerperal convulsion, and a demonstration likewise of the rapidity with which uræmia may be developed.

So far as I know, no such crucial test has ever been followed by such a result, and thus the argument falls short of a demonstration. But the common consent of experience is contented to suspect the existence of uræmia when the objective symptoms of uræmia are manifest, and the presence of albumen is not sought for till the objective symptoms appear. Is this contentment a righteous and a scientific repose? It is certainly within the range of the common experience that at various epochs along the pregnant period convulsions have appeared after an absence of all the antecedent objective symptoms; or, it may be, with such a slight and short preface of one or more of them as not to attract serious attention at the time; or, perhaps, only recalled by an effort of memory after the event. Is it to be assumed that the tests for urea and albumen would have revealed the threatened danger days or hours in advance? Are there any cases—is there an isolated case—where, in the absence of the objective symptoms, there has been discovered in the urine of the pregnant woman albumen or a deficiency of urea, when, upon this discovery, skillful treatment was instituted and convulsions ensued, despite the treatment? I grant that, in this case, the attendant's conscience would be at rest; and I would further grant that one would have to face this question seriously as to his duty in the guardianship of any primipara.

I leave altogether untouched the questions cognate to this inquiry, and I forbear to introduce the cases at my disposal, because of the limited time, and because they are open to the same criticism that applies to the general argument, that the absence of the objective symptoms are not sufficient evidence of the absence of uræmia. Grant that "convulsions are less likely to follow when albuminuria is chronic than in the rapidly-produced form; that where it has existed nearly through the whole of pregnancy the system seems to accommodate itself, and the irritability of the nerves is not intense enough to excite convulsion;" grant "that the sudden invasion is the exciting cause," even then there are no data out of which to formulate a statement as to the conditions under which uræmia can be developed in the pregnant woman, and the rapidity with which it can be excited. Grant that the presence of the objective symptoms is the signal for anticipated convulsion, the fact remains that the absence thereof is occasionally followed by convulsion. The convulsion comes. Albumen is found in the urine. What provoked this sudden onset? Frequently the cause is discovered after the event.

This brings this inquiry directly to the object of this discussion, viz., to discover the prophylaxis.

Granting the possibility or certainty of the unannounced puerperal convulsion, has experience any lesson? Has the physician any resources which he can turn to account? Has he any means for knowing that what he might do would avert the disaster? Has he any ground for intelligent action? It is easy to say all manner of things that bear upon general hygiene, the proper performance of all normal functions, and the avoidance of all imprudence and excess. Granted; and when that is done and said, what then? No one could question the duty, the imperious necessity, of interfering with all the suggestions of experience, upon the slightest manifestation of any of the objective symptoms of uræmia. But, in their absence, there is left but anxiety that the unexpected may happen, and the earnest preparation to meet it if it come. It may be pronounced squeamishness or neglect of duty on the part of a physician who would hesitate, in the absence of all objective symptoms of uræmia, to require more or less frequent examinations of the urine of all his pregnant primiparæ, on the ground of needless interference and idle suggestion of danger; but I heartily agree with the sentiment of one of the speakers at the last meeting, that to turn the primipara's attention to a watchfulness for symptoms that may never present, is to risk increasing a hyperæsthesia already too highly exalted. Every precaution that a skilled prudence can suggest should be taken. A strict observation is a duty. The details of that observation must, in each case, be regulated by the conscience and the appreciative instincts of the attendant.

To summarize the belief of the speaker as compressed in these few moments :

- (1). With any recognized use of terms, a non-uræmic puerperal convulsion is not known.
- (2). The presence of the objective symptoms of uræmia in the pregnant woman demand interference, to avoid, if possible, the recurrence of the convulsion.
- (3). The absence of the objective symptoms of uræmia does not preclude the possibility that the pregnant woman may be in a condition ripe for the rapid development of uræmia under an exciting cause, but there is no way yet known of estimating the chances of that possibility.
- (4). The pregnant diathesis disposes to the onset of convulsions in an acute uræmia.
- (5). In the present state of knowledge of the subject, there is no prophylaxis against the unannounced puerperal convulsion.

DR. A. R. MATHESON: In connection with the discussion of the interesting subject of puerperal convulsions, I have prepared a few observations from the notes of nine cases that have come under my own notice.

NO. 1.—Primipara; 22 years of age; born in the United States; had complained of headache and dizziness for several weeks prior to labor; was seized with convulsions soon after being delivered of a healthy female child, and continued, with slight interruptions, for twenty-four hours, when she was abandoned by her physician, who refused to devote any further time to her case, as he deemed her condition entirely beyond medical aid.

I saw her shortly afterwards and found her suffering from convulsions of a clonic type, relieved by a period of about half an hour, in which she was profoundly comatose. On inquiry, I learned that she had had œdema of the limbs and headache for several weeks, and severe epigastric pain some hours prior to the occurrence of labor; that her labor was not tedious for a primipara; that she bore it courageously and without an anæsthetic; and that there was scarcely two ounces of blood escaped; also that

she had not passed any urine since the completion of labor; her bowels had moved twice, but imperfectly. I introduced a catheter and succeeded in getting less than an ounce of smoky urine, which with nitric acid boiled almost solid. My predecessor in the case had used chloroform during the convulsions, but further than this did nothing. I continued the chloroform, and in addition, proceeded to act upon the alimentary canal by the exhibition of two drops of croton oil and a quarter of a grain of elaterium. In a short time the bowels moved copiously; the convulsions became shorter and the coma less profound, and I had the satisfaction four hours afterwards of drawing about four ounces of urine. Artificial heat applied to the body had produced a free perspiration. Twelve hours after my advent in the case she had had eleven convulsions with a respite of three hours, in which the stertorous breathing of the coma had modified and she had glided into a sweet and peaceful slumber, from which she opened her eyes and looked around her and swallowed some milk and brandy, and again fell into a quiet sleep which lasted for several hours, when she again awoke, and during the next forty-eight hours suffered from violent puerperal mania, which terminated with her death; a result that has caused me many misgivings, as I had entrusted (while resting myself) the administration of a mixture of chloral and bromide of potassium to her husband, who made some claims to a knowledge of medicine from being a druggist. And I have sometimes thought whether in his anxiety he had not given the mixture more frequently than directed, and precipitated the fatal issue. Death occurred unexpectedly and apparently from syncope.

No. 2.—Primipara; 17 years of age; born in England; during the eighth month of pregnancy, and while apparently in the enjoyment of good health, was seized with convulsions of a tonic type which speedily induced labor, and as the os was easily dilatable, it was only a short time before the forceps were applied, and either myself or my friend, Dr. Skene, I cannot recall which, delivered her of still-born twins. There was scarcely any blood escaped; she died in the fifteenth convulsion, about an hour after delivery, apparently from acute asphyxia.

Prior to the rupture of the membranes she passed urine, but I did not secure any of it.

No. 3.—Aged 32 years; born in Brooklyn, fourth pregnancy; complained for two months of headache, dizziness, dark spots before the eyes and swelling of the extremities; was taken with convulsions during the eighth month of pregnancy. I saw her during the third convulsion; used chloroform during convulsions; free catharsis from croton oil and elaterium; dry cupping over the region of the kidneys hastened labor by rupturing membranes; manual dilatation and forceps; male child; lived six hours and died in convulsions. Mother had four convulsions after delivery—thirteen altogether—and made an excellent recovery; urine was albuminous (made no memorandum of amount).

No. 4.—Multipara; saw in consultation with Dr. Buel; she had had œdema of the extremities and been in poor health about three months; was taken with severe epigastric pain about three o'clock in the afternoon, and about ten o'clock uterine contractions came on, simultaneously with convulsions, and she was delivered of a living child about 11:30 o'clock.

There was complete suppression of urine. Chloroform was used during the convulsions; the bowels were moved freely. Enemas of chloral hydrate were employed, but did not appear to exert any beneficial influence, and it seemed to me that the convulsions were more protracted and tonic after its use. With the good counsel of Drs. Ford and Kissam, many other remedies were employed, but without any benefit, and she died twenty-four hours afterwards from asthenia.

No. 5.—Born in the United States; 25 years of age, fourth pregnancy; was enjoying robust health until the eighth month, when she was seized with violent epigastric pain and headache, which continued for several hours, when she was taken with a convulsion. I immediately sent for Dr. Skene, and by his advice I introduced a catheter and drew off half a pint of bloody urine; she remained comatose for half an hour, and died during the second convulsion.

No. 6.—Multipara; 38 years of age; born in Ireland, eighth pregnancy; was in excellent health up to the date of the first convulsion, which took place shortly after the beginning of labor. I saw her during the second or third convulsion; she was a very large woman, weighing, I should think, two hundred and twenty pounds; her face was of a mahogany color. I immediately opened the median cephalic vein and drew off over twenty ounces of blood. The catheter was used, but a little water was found in the bladder which was very albuminous. A half grain of elaterium, with two drops of croton oil, mixed with a little butter, was placed on the back part of the tongue. She had but two convulsions after venesection; she was delivered of a still-born child, and made an excellent recovery; no traces of albumen three days after confinement.

No. 7. Primipara; 37 years of age; born in Ireland; at full term was taken with convulsions, about five o'clock A. M. Dr. Maddren saw her soon after and administered fld. ext. verat. viridis subcutaneously, in ten drop doses; used chloroform during convulsions. I saw her in consultation about eight o'clock; we administered croton oil and elaterium, but without getting any response. The membranes were ruptured before the arrival of Dr. Maddren, and the os was dilated about four centimeters in diameter; there were no contractions, auscultation over the thin walls of the abdomen—the patient being but poorly nourished—failed to elicit any uterine souffle, or the sound of the foetal heart, and being convinced that the foetus was dead, we determined upon hastening delivery by dilatation and cephalotripsy. No convulsions occurred after delivery; some urine obtained with the catheter was highly albuminous; she made a good recovery, and has since been confined without any accompanying trouble.

No. 8.—Twenty-eight years of age; born in the United States, fifth pregnancy; was in excellent health up to the time of confinement; urine, examined a few days before, was normal; labor begun at 5:30 P. M., and was completed at 6:30 P. M.; placenta expelled while I was tying the cord; did not administer ergot, as is my custom, as the uterus contracted firmly. I remained nearly an hour, and heard from her at ten o'clock P. M., as I had directed. She was doing nicely, and said the after pains were not severe enough to require an anodyne. At two o'clock A. M., I was informed by a messenger that she was in convulsions. When I arrived at the bedside she was asleep. She soon opened her eyes, and expressed surprise at my presence. Her appearance was so natural, her language so correct, that I felt that the attendants had been mistaken; but I was only a moment in doubt, as she went into a convulsion, which was succeeded by another, and another, until she finally succumbed at five o'clock P. M. the following afternoon. The secretion of urine had been almost entirely suppressed, but what could be obtained was highly albuminous. In the care of this case I was ably advised and assisted by Drs. Skene, Harvey, and W. H. B. Pratt. Chloroform catharsis, verat. viridis, jaborandi, cupping, wet and dry, were all used in their appropriate places, but without in the least ameliorating the force or character of the convulsions.

No. 9.—Primipara; 22 years of age; born in Germany; in labor nine hours; delivered of a healthy child; and was attended by a midwife. She was taken with convulsions a few minutes after delivery. I saw her during the third convulsion. Used

chloroform and four minims of fld. ext. *verat. viridis*, subcutaneously; and, at the suggestion of Dr. Stuart, fifteen minims of sol. morph. magd. She had six convulsions in all. Urine, drawn about four hours after the last convulsion, gave about 10 per cent. albumen; no casts; sp. gr. 1015. Twenty-four hours after no albumen. She made a good recovery. During the four hours preceding delivery the midwife informed me that she had given four spoonfuls of Squibb's fld. ext. ergot, which, by actual measurement, amounted to $\frac{1}{3}$ V.

In answer to the questions of the circular the 1st, 3d, and 4th had precursory symptoms.

The first received no prophylactic treatment; 3d was carefully treated by Dr. Buel; 4th did not consult a physician; 2d, 5th, 6th, 7th, 8th, 9th, no precursory symptoms; 3d, etiology; 1st, 3d, and 4th, uræmic; perhaps some of the others.

In reference to the 9th the question has arisen in mind: Were the convulsions in this case due to the action of the ergot in producing vaso-motor spasm, with a consequent rise in the arterial pressure?

4th.—Propriety of hastening delivery?

I shall do so when it is practically possible, until I am better instructed.

5th.—Make it a rule to examine the urine of all my cases.

Patients never offer an objection; if they should I would not attend them.

DR. W. WALLACE: I have to report seven cases of puerperal convulsions occurring in my own practice. In addition to these, I believe I have seen fully as many more in the practice of my friends. In none of these cases was there any treatment pursued previous to confinement, for the reason either that the case was not seen till labor set in or else that the occurrence of convulsions was not looked for. I may here state that very seldom, indeed, in my experience, are there any precursory symptoms of puerperal convulsions.

Of these seven cases two of the mothers died and five recovered. The two who died were not seen till some time after convulsions began, and both were apparently in a hopeless condition from the first. In one of these women it was the 12th labor and about six weeks before term; in the other it was her sixth labor and likewise premature. The first of the two died eight hours and the other about 12 hours from the commencement of sickness. The five cases which recovered occurred in *primiparæ*.

In all the seven cases delivery was accomplished. Albuminuria was recognized in four of the seven cases. In one no examination of urine was made, but from the general appearance of the patient, no doubt existed as to the case being uræmic in character. In the remaining two cases no albuminuria was present. In both of these cases the convulsions were probably reflex in character.

I have thought right in this connection to report the notes of four cases where albuminuria was present during pregnancy, in some of them for several months before commencement of labor; and in none of these four cases of albuminuria did convulsions occur.

Three of the four cases were under treatment, and all these presented well-marked signs and symptoms of disease of the kidneys.

I have before me a brief resumé of the history of the eleven cases, and with your permission I will read them:

1. Mrs. N., æt. 36; 12th labor. In this case there was no examination of the urine, but from the general dropsy and other symptoms, I have no doubt it was a case of

uræmic poisoning. This woman was in convulsions when I first saw her at noon. She had had three convulsions before I saw her, was comatose and extremely restless, requiring considerable force to prevent her getting out of bed. On vaginal examination the uterus shows no signs of labor commencing. At 4 P. M. I again saw her, still comatose and no indications of labor. 7 P. M. Os admits two fingers but is hard. Dr. Dudley saw her with me and agreed that if dilatation could be effected delivery should be accomplished as soon as possible. I succeeded more readily than I anticipated, and in about 15 minutes had removed the child by *version*—it was *dead*. The mother died about an hour afterwards.

2. Mrs. L., æt. 36; 6th labor. This woman had general dropsy for some weeks. Was called to see her in convulsions. When these ceased she remained comatose. Did not expect her confinement for a month. At second visit found her babe dead between her legs. Probably born during a convulsion. She died eight hours afterwards.

3. Mrs. C., æt. 22; primipara. This patient was not seen till labor began. Convulsions commenced during first stage. She had three severe attacks before delivery took place, and eleven afterwards. Was for 36 hours comatose. Made a good recovery. Boy born alive; delivered by forceps. Copious albumen and general dropsy.

4. Mrs. R., æt. 23; 1st confinement. When sent for, was told patient was in convulsions. On arrival, found her completely comatose, and, while making vaginal examination, another convulsion came on. Gave chloroform, and dilated digitally, and in half an hour applied forceps and delivered her of a living child. After delivery there were no more convulsions, but coma continued complete for 30 hours. In this case *no albumen*.

5. M. M., æt. 21; primipara; in labor 40 hours; in convulsion 3 hours. Os but slightly dilated, but under influence of chloroform digital dilatation was so effectual that in 15 minutes I was enabled to get my hand in uterus and perform *version*. Her babe was born alive, and she made an excellent recovery, although she had three convulsions after delivery. *Albuminuria* present.

6. Mrs. L., æt. 25; primipara. Patient had one convulsion when head was on perineum. Delivered with forceps. One and a half hours after delivery, had another convulsion, which lasted about 3 minutes. Chlorof. given during convulsion, and followed by morph. and potass. bromid. Child born alive. Good recovery. No albuminuria.

7. Mrs. S., æt. 19; primipara. One severe convulsion while head was dilating external parts. Gave chlorof. and applied forceps. No albuminuria. Good recovery—a living child.

8. Mrs. H., æt. 24; primipara. Chronic albuminuria. Cardiac enlargement and valvular disease. General dropsy. Prematurely delivered of a putrid child. No convulsions. The mother died about a month afterwards.

9. Mrs. C., æt. 24. This patient has very suspicious appearance, and up till three weeks ago there was much albumen in urine, together with general dropsy. Her mother states that frequently of late she has had hysterical fits. She was delivered by forceps of a living child, and had no convulsions.

10. Mrs. R., æt. 34; 2d labor. This patient I very closely watched, for the reason that she had been under my care since her previous confinement, suffering from albuminuria. Her urine became free from albumen when about four months gone. She has been in fair health from that time till now. Labor was very tedious, but there were no threatenings of convulsions at any time. Her babe was still-born, although 20 minutes before its birth I heard its heart quite distinctly.

11. Mrs. G., æt. 34; 6th labor. This patient came under my care in April, when between 2 and 3 months pregnant. She then was extremely pale, and had general dropsy. Urine was loaded with albumen; granular casts and blood corpuscles were present in great abundance. Her sight had been failing for 2 years. I sent her to Dr. Prout, who kindly examined her for me. He reported marked albuminuric retinitis in both eyes, though more advanced in one than in the other. Patient continued under treatment, varied from time to time, and in September I found her urine healthy, both chemically and microscopically. Dr. Prout again examined her in November, and reported. I quote from his note to me: "There has been improvement in vision as well as in ophthalmoscopic appearances. I found in May hemorrhages in the retina, and a white patch (of fatty degeneration), such as we find in renal troubles. These are no longer visible. There is now merely a doubtful look, that might pass muster for normal, if one were not looking for evidences against a normal condition. In the left eye there are some of these same doubtful appearances. Vision in the right eye is better. It was $\frac{20}{30}$, it is now $\frac{18}{20}$. In the left it is not so good—it was $\frac{20}{30}$, it is now $\frac{18}{40}$. For this deterioration I can see no satisfactory explanation. If I knew nothing of the case (he says, in conclusion,) now I should not suspect renal trouble." This patient went to full term, and was safely delivered by forceps, without the use of any anæsthetic, of a large, living boy. No threatening of convulsions.

This, then, completes my list of cases.

What conclusions can be drawn from this record with reference to the five topics suggested by Dr. Segur's paper:

1st. In what proportion of cases are there precursory signs? In my cases no signs that I could recognize manifested themselves. In five of them the patients were either comatose or had had convulsions before I saw them.

2d. I know of no precursory signs.

3d. Etiology. Five of the seven were uræmic.

4th. I believe in hastening delivery by the means apparently most suitable to each individual case.

5th. Preventive therapeutics. Although I have made the statement that there are no precursory signs, I look upon the presence of dropsy, accompanied by albuminuria, as a condition that may give rise to convulsions; and, consequently, whenever I have the opportunity, I put the patient on appropriate treatment. My experience in this is quite satisfactory, inasmuch as I have herewith reported four cases of well-marked albuminuria under treatment for varying periods, where each went to full term and had no threatenings of convulsions.

DR. J. H. H. BURGE: The late Dr. Wade sent for me in the night to deliver at full term a lady who had been in convulsions more than an hour and who was quite insensible when I arrived. The doctor was suffering from rheumatism in the right wrist and requested me to use the forceps. There was no difficulty whatever in the delivery. Hemorrhage free, but not at all excessive. The urine was highly albuminous, as was ascertained the next day. The amount of albumen gradually lessened. There was no return of the convulsions, and health was fully restored.

2. In the winter of 1860, Dr. W. H. Gardner requested me to see with him a primipara who had been in active labor twelve or fourteen hours. Membranes long since ruptured; convulsions constantly recurring and lasting generally as long as the intervals. The doctor had made an ineffectual attempt to apply the forceps. The pelvis was small and the os by no means fully dilated. I advised delay, careful administra-

tion of chloroform, which was already in use, and a large injection of hop tea into the rectum. Delivery was effected about six hours later. Urine highly albuminous. The patient did not fully recover consciousness for three days. The hop tea enemas were repeated from time to time, as they seemed to lessen the frequency of the convulsions, which, for more than thirty-six hours, were recurring at irregular intervals. The other agents used in the treatment of this case were elaterium and bromide of potassium. The patient recovered very slowly, but entirely.

3. In June, 1862, a short, stout, young primipara had been all day in active labor, when suddenly she was attacked with convulsions so marked in character and violence that I thought she would certainly die at the time. There had not been a hysterical manifestation. My instruments were not at hand, and it was more than half an hour before I procured them. The delivery was easily effected. There was no return of consciousness during this delay, but a few hours afterward the patient was as bright as if nothing unusual had happened. There were no more convulsions, and a better "getting up" I have never seen.

It will be observed that in neither of these cases was phlebotomy resorted to, so that if loss of blood was one element in the favorable result, it was only such loss as occurred naturally in the parturition. This was by no means unusual, except in one case, and not excessive in that. These cases were also treated without *veratrum viridis*, as they all occurred before the grand discovery of the therapeutic value of this agent was made and confirmed by Brooklyn physicians.

4. In September, 1878, Mrs. D., 454 Henry Street, was taken suddenly ill with nausea and slight sanguineous discharges, she being at the seventh month of her second pregnancy. Headache and slight œdema of lower extremities had given some apprehension of approaching trouble. A brisk cathartic was given, but before it had time to take effect convulsions came on. Premonitory labor pains had commenced, and they were encouraged by gentle artificial dilatation of the os-uteri. Dr. Bunker was invited to consult with me. We quickly and positively decided that, as the pulse was feeble and not rapid, *veratrum viridis* was out of the question. A large enema of hop infusion was given to hasten the alvine evacuation, and the labor was carefully hurried and completed with the forceps at the end of four hours. Hypodermic injections of pilocarpine ($\frac{1}{8}$ of a grain) caused excessive ptialism, so that it became necessary to mop the saliva from the mouth and to place the patient in a position favorable to its discharge, in order to prevent drowning. At the end of twenty-four hours she became perfectly conscious, and was greatly surprised at her surroundings. The renal secretion, however, was very scanty and very albuminous, and the convulsions returned after a remission of two or three hours and continued until she died. The autopsy revealed both kidneys atrophied; capsules strongly adherent; other organs sound.

The proportion of cases of puerperal convulsions in which precursory signs occur is probably much greater than the proportion in which those signs are recognized.

In general terms, I should say the hysterical, and perhaps nearly all the non-uræmic, cases are without special premonition. Again, a certain indefinite proportion of truly uræmic cases are so acute as to have no antecedent history, being dependent upon a sudden renal congestion or inflammation, from exposure to cold, or from simple uterine pressure.

The precursory signs, when they do occur, are vertigo, *muscæ volitantes*, dimness of vision, cephalalgia, confusion of ideas, œdema, more or less extensive, albuminuria, etc.

I will not venture to say anything upon the etiology of puerperal convulsions, but leave the subject to those who have given special attention thereto.

As to the propriety of hastening delivery, I suppose this is universally conceded when the convulsions have already commenced. When they only threaten, there is room for the exercise of judgment as to the amount of justifiable delay. This should depend on the period of gestation, the severity of the premonitory symptoms and their persistency, or, on the other hand, upon the readiness with which they yield to medical treatment.

Among the preventive measures, in all cases in which the precursory signs or the premonitory symptoms are well marked, I would enumerate free, but carefully guarded, catharsis, protection from extremes of heat and cold. Digitalis, as a tonic, given two or three days at a time, and if marked labor be not experienced, induced labor, especially when the child is viable, or, on the other hand, when we have reason to suppose it dead.

DR. W. G. RUSSELL: I have personally attended five cases of puerperal convulsions, in three of which there were precursory signs, and in the other two not.

The signs in the three first were swelling of the face, eyelids, hands, and extremities, and almost constant headache. Very little urine voided. Impairment of vision was another marked symptom in the cases where there were premonitory symptoms.

Uræmia seemed to be the cause in the three cases. I could not discover any cause in two of my cases except it was loss of nerve power, superinduced by prolonged labor.

The cases in which there were precursory signs were not seen by me early enough to decide as to the propriety of hastening delivery. I have seen the most happy effect from preventive therapeutics.

DR. J. A. BRODIE: I have no notes of my convulsion cases, but will relate from memory my experience in a case that I attended a short time ago.

Mrs. F—, æt. 35, was seen by me one week before her first confinement; had been vomiting every day, she said, for several months, and had not consulted a doctor about it, because her friends said it was healthy for her. She had frequent small diarrheas, but appeared well nourished, had no headache, and no disordered vision. I stopped the vomiting with ipecacuanha. Her urine was normal in color, and at the minimum normal quantity; no albumen.

Her labor was natural, and all went well until the fifth day, when fever appeared; breasts full but not painful. Early in the morning of the sixth day she was taken with convulsions and coma. After giving her a cathartic I bled her three times freely during the fits (thirteen in number), which only reduced them in force and frequency. Next I gave chloroform inhalations, and afterwards morphine, hypodermically, but soon abandoned them for veratrum viridis, and fluid extract gelsemium, which I gave in alternate half teaspoonful doses, per rectum, as her stomach was again irritable. After taking about two teaspoonfuls of the remedies in that way, symptoms of collapse appeared, but she soon rallied, and had no more fits afterwards.

The subsequent delirium, which was severe, was controlled by gelsemium and nourishing diet. She is living and well, but with a slightly impaired memory. Were the convulsions but the reflex of the long continued gastric irritation, together with the other irritations necessarily following confinement? I leave the question for your more experienced judgment to decide. The published discussions on this subject are very important, and I read them carefully, so that any predisposition to ill-judged routinism in my mind, relating to these cases, may be removed. Had I *the same case* to treat again I would rely more on veratrum, and other sedatives, and less on bleeding.

DR. C. VROOMAN: Three cases of puerperal convulsions have come under my care:

No. 1.—Complained of headache and general malaria for a week previous to the attack. Albuminuria and dropsy also existed. Patient seven months pregnant and no signs of labor. Headache intense for six hours preceding first convulsion.

Treatment: Venesection and chloroform during first convulsion; verat. virid. tinct. (Norwood's) 1 drachm, every hour afterwards, till pulse was reduced to sixty. Three or four convulsions before this result was obtained. Six hours after first convulsion patient placed under influence of chloroform, and the os uteri partially dilated. For the next twenty-four hours kept the patient's pulse below sixty with verat. viridis, using altogether about 2 ounces of Norwood's tincture, while waiting for expulsion efforts of uterus to be established. Immediately after delivery, when for about two hours attention to condition of patient's pulse had been interrupted, and no veratrum given pulse increased to 120, and a slight convulsion succeeded. Subsequently, for two days smaller doses of veratrum were given, and patient made excellent recovery.

I have since learned that this patient, during subsequent pregnancy, had albuminuria, and died from puerperal convulsions during her confinement.

Cases Nos. 2 and 3 were similar, and may be mentioned together. In both was there general malaria, and severe pain in the head for some time preceding the attack. Both occurred during confinement at full term. In neither of them could we ascertain whether albuminuria existed. If any anasarca existed it was but very slight.

Both of the patients developed symptoms of effusion into the brain, and both died, I believe, from organic lesions in the brain, resulting from the violence of the puerperal convulsions. In both cases was delivery hastened.

I will make no comments on these cases, but would observe that in all pregnant patients who have albuminuria I endeavor to promote excretion, and reduce congestion of the kidneys, on the theory that uræmia exists, and fearing the supervention of puerperal convulsions.

DR. C. L. MITCHELL: I cannot tell precisely the number of cases of puerperal convulsions that I have attended, or that have come under my personal observation: they probably amount to about twelve. Of these, two occurred while the patients were apparently in good health; and in both these cases the convulsions were due to errors in diet. There were no precursory signs.

In the remaining instances there were present more or less œdema, or violent pain in the head, or other uræmic symptoms.

The causes have been, as stated, gastro-intestinal irritation or uræmia.

I think it proper to hasten delivery where uræmic symptoms are present in a serious degree, and relief cannot be obtained by appropriate treatment. I do *not* think it proper to wait till the symptoms show that convulsions are *imminent*.

In the way of prevention I have advised the exclusion of every form of *meat* as an article of food; the thorough and *certain* clearing of the bowels; the most careful avoidance of every possibility of taking cold; of suddenly checking perspiration, or being in any way so exposed that the patient feels cold; and, finally, when convulsions have once occurred, the avoidance of future pregnancy until all evidences of abnormal kidney function have long disappeared.

DR. L. C. GRAY: There is no pathology to a convulsion that can be appreciated post-mortem, as are the morbid changes in a diseased lung, or a diseased heart, or a diseased intestine. The molecules of a nerve-cell are more mobile, excitable, more

liable to undergo sudden and violent changes than the molecules of any cell in the body. We should understand this fact first and foremost. Each nerve-cell has going to it fibres which conduct sensation, and fibres going from it which conduct motion. These highly excitable, mobile molecules have, therefore, excellent highways along which can pass irritations from a thousand different points to them, and they have equally excellent highways along which they can project their agitated condition and have it expressed by a thousand different movements.

In conditions of the nervous system in which convulsions occur, we have simply these nerve cells, the molecules of which are highly excitable and mobile in the normal state, rendered still more unstable by disease, so that irritations which would ordinarily impinge upon them harmlessly, now stir them up to sudden and violent molecular changes that are outwardly evidenced by the muscular movements, to which has been given the name of convulsion. Exactly what it is that causes the molecules of the nerve-cells to become so highly mobile we do not know. We simply know that blows on the head, certain diseases and certain deformities will bring about this condition, but how they do it we do not know; and after the death of persons who have suffered from convulsions for some time, we find various lesions, but they are effects and not causes.

Neuro-pathologists have been on the search for some group of nerve-cells so centrally situated that disturbance of them would give rise to all the wide-spread phenomena of a convulsion, and they claim to have found what they sought in the medulla oblongata, in the so-called "vaso-motor centre." But this is a pure theory, and a poor one at that; for it is by no means proven that there is any such centre, and if there were, dilatation and contraction of vessels would not explain all the phenomena. It is possible that there may be some such centrally-located cells as yet unfound; or the convulsive movements may be the result of sudden molecular change in great tracts of cells. In the pregnant woman, wonderful textural changes rapidly take place in one organ—changes which have no analogue in any other organ of the body. As these textural changes are regulated by the cells of the sympathetic system, the ganglia of this system have an enormous additional task thrust upon them. Their cells ingest and egest more, the molecular changes are greatly quickened, and thus the natural instability of the nerve-cell is largely increased. From the particular ganglia in the lumbar region, which "preside" over the uterus and its appendages, the excitement proceeds along connecting strands to the other ganglia of the sympathetic and to the spinal cord and brain. Hence the vomiting, the quickened pulse, the variable appetite, the cutaneous pigmentary alterations, the emotional state, the general nervous tension, of the pregnant woman. Hence, also, if we may rely upon the investigations of Drs. Da Costa and Longstreth, of Philadelphia, the albuminuria so often encountered. These gentlemen, in an article published in the July number of the *American Journal Medical Sciences*, detail their examination of a number of cases of Bright's disease, in all of which they found degenerative changes in the renal ganglia, the cases ranging from the slighter forms to the most chronic ones.

This article is, in my opinion, the most valuable and original ever published in America, and embodies a discovery almost as important as the original one of Bright's. The nervous cells of the pregnant woman, then, are in a highly mobile condition; are, to use the phrase of an electrician, "highly charged," and, like a dynamite molecule, they may explode at any moment. Albuminuria constitutes no warning; it is simply evidence that the renal ganglia are feeling the effect of the general excitement. In speaking of treatment, I am, perhaps, overstepping the proper limits, inasmuch as my experience in puerperal convulsions has necessarily been much less than that of many

who have spoken, and I can only speak from my experience of nervous diseases in general. The object should be to lessen this nervous instability in every pregnant woman. I can endorse what Dr. Chapman has said, in regard to treatment, with some modifications. In most cases of functional nervous disease you will find uric acid in excess in the blood, and the digestion and the powers of assimilation are impaired; these processes do not go on with the same facility as in health. If, in these individuals, the digestion is made as easy as possible, and, consequently, the assimilation, you diminish the amount of work that is thrown upon their ineffective nervous systems; and if, in addition, you use such remedies as may neutralize the excrementitious substances in the blood, you add greatly to your therapeutics. In almost seventy-five per cent. of neuralgic patients, the most effective treatment will be a small dose of calomel, followed by a saline, and then small doses of the alkalies, largely diluted, at the same time that the patient is put upon a diet of milk, farinacea, and fresh fruit. As to the explanation of this mode of treatment—as to whether its efficacy is due to the fact that this diet is non-nitrogenized, that is too large a subject to enter upon here; but I have no faith in this explanation. Milk is largely nitrogenized; and in some cases where a farinaceous diet has become distasteful, so that an insufficient quantity of food is taken, we can wheel about and give the patient a delicately-nitrogenized diet, with the happiest effect. The true reason is that digestion is made easy. After awhile, in certain of these cases, we may find it necessary to strengthen the nervous system, and enable it to aid the digestion with healthier nervous influence. For this purpose we have no better remedy than quinine. Applying these and kindred facts to the subject under discussion, I should treat the pregnant woman thus: Regulate the diet carefully, so that a plenty of easily-digested food may be taken, such as milk, farinacea, fresh fruit, and, if necessary, a little meat and vegetable; keep the bowels regular; give small doses of the alkalies, as bicarbonate of soda, largely diluted; if albuminuria exist, take care that it do not accumulate, and use iron to obviate the anæmia arising from it; if there be evidence of nerve-cell excitement, give quinine in tonic doses to the weakly woman; give the bromides in moderate doses, often repeated, to the robust woman; and give a combination of quinine and bromide to the woman who is weakly, and at the same time very “nervous.” In the convulsions, tincture *veratrum viridis*, the bromides, and chloral hydrate, are all we can use. Blood-letting is, I think, simply barbarous. It has not been proven to be as efficacious as the remedies just mentioned, and it deprives a woman of what she cannot spare. Of course, albuminuria may necessitate purging. There are others here who can state much better than I when delivery should be made; but, logically, it should only be brought about when it is probable or certain that the patient’s life demands that an end should be put to the original uterine process, which is the source of all the mischief.

DR. J. J. LAMADRID: On the 4th of April, 1877, saw Mrs. C., aged 18, a patient of Dr. N. Matson, and found she had been insensible for a few hours previous, and was just getting over an eclamptic attack, of which she had had six, with an interval of about fifteen minutes, and of three or five minutes’ duration. Patient was a primipara, eight months pregnant. Her general health had been poor previous to and for some time after her marriage, a year ago; in fact, at that time she was subject to chorea. The chorea, however, disappeared soon after she became enceinte—a fact rather remarkable, I think, for one in her condition.

As might be expected, she had a very anæmic appearance, and was somewhat emaciated; but no œdema in any part of the body. Lately her appetite had been poor and

capricious, and her bowels inclined to be costive. On the other hand, she had been free from headaches up to that day; in the morning, however, her mother said she complained for the first time of pain in the head, and said that she had not felt well for the last two days. Dr. Matson had already seen the patient, some time during the day before I did, and had prescribed ten grains each of bromide of sodium and chloral hydrate in solution every two hours. She had already taken three doses when I reached her house. Soon after my arrival I gave her twenty grains more of chloral hydrate. She had no sooner swallowed this than she went off suddenly into another severe attack, during which her body became much distorted, face livid and lips and tongue almost black, with foam at the mouth and a peculiar rattling noise in the throat. The pulse was full, 108 per minute. Ordered applications of ice-water to the head, and hot bottles to her feet.

During the remission, on examination found the uterus much anteverted, the cervix very high and difficult to reach; but by using my left index, I succeeded in bringing down the latter into the line of the vagina and soon was able to pass my finger through into the uterine cavity. While making continuous traction upon the anterior lip of the os for the purpose of forcible dilatation, and effecting delivery either by version or the forceps, another attack took place, which, for the time being, prevented me from accomplishing my object. I immediately placed the patient under chloroform. Chloral hydrate was again given (grs. xxx.), just half an hour after the last dose. At this time Dr. Matson arrived, and we deemed it advisable to continue with my previous efforts to induce premature labor, to deliver as speedily as possible. By so doing, we thought this might put a stop to the convulsions, and her life might be saved, which at the time seemed very doubtful. Chloroform was therefore reapplied, and I then introduced the smallest of Barnes' dilators; but this Dr. M. thought was too slow work, and after a while decided to use his own fingers, which he believed would answer the same purpose, if not in a shorter time, fully as well. Just before the chloroform was readministered a teaspoonful of Squibb's fluid extract of ergot was given, to act on the womb and thereby help rapid delivery. After an hour of steady hard work and perseverance, during which the membranes were ruptured, the os-uteri, which proved to be very rigid at first, was at last considered sufficiently dilated to admit the forceps, and Hodge's forceps were applied at once by Dr. M., and the child was delivered in a short time without much trouble. As there were yet some feeble pulsations felt in the cord, attempts were made to resuscitate the child, but failed, for the pulsations soon ceased altogether. The placenta speedily came away, and the little hemorrhage which took place immediately after its expulsion was thought trifling, considering the nature of the case. At this time another teaspoonful of ergot was given, and by firm but gentle compression over the hypogastric region the uterus contracted well. Soon after the chloroform had been withdrawn patient had one more convulsion, and it was again used. After that she slept quietly for nearly an hour, at the end of which she had another severe spasm, which soon yielded to the chloroform. We left at half-past seven P. M., two hours and a half after parturition. Patient then was sleeping. The mixture of sodium bromide and chloral hydrate was ordered to be continued (20 grains of each) every one, two or three hours.

At 11 P. M. we saw the patient again; was unconscious; had had two more convulsions (controlled by chloroform); last one, however, very light—pulse 130, temperature 103 F.

Treatment: Ordered two grains of quinine every two hours, and fluid extract veratrum viridis, gtt. viii., to be given in two doses two hours apart.

April 5th, 9 A. M.—No more convulsions, but still unconscious. pulse, 120; tem-

perature, $102\frac{1}{2}^{\circ}$ F. Some tympanites, for which turpentine stupes to the abdomen and an enema of the same were ordered; also quinine to be continued as before. As the patient had not passed urine since delivery, a catheter was used; the urine was found highly albuminous, but no casts.

At 9 P. M.—Complete consciousness had returned, and although the patient recognized her friends, she had not spoken a word, and seemed to have lost her power of speech, as she would not make any answer to a few of our questions, but would readily nod Yes or No with her head; thus it was she told us of some pain and tenderness in the abdomen, for which was prescribed: Tinct. opii. deodorat, gtts. xx., every two or three hours. Pulse and temperature about the same as in the morning.

April 6th, 9 A. M.—Patient is able to talk, and complains of great soreness and pain in the abdomen. On examination, found great tenderness and tympanites, especially in the hypogastric region. Pulse, 124; temperature, $102\frac{3}{4}^{\circ}$ F. Tongue very much coated, and discharge per vaginam very offensive. She also complained of ringing in the ears, produced no doubt by the quinine. The latter was ordered at longer intervals, and in addition iron, for the purpose of making up the deficiency or loss produced in the red corpuscles by the increase in the elimination of albumen. To assist the iron, nux-vomica and sweet spirits of nitre were prescribed in combination as follows: R. Tincturæ ferri chloridi, tincturæ nucis vomicæ, $\mathfrak{f}\text{℥iv}$. of each; spiritus ætheris nitrosi (Squibb's), $\mathfrak{f}\text{℥iii}$.; misce, fiat mistura. Dose: Teaspoonful in plenty of water three times a day.

Continued with the opium in gtts. xx every two hours; also hot fomentations to the abdomen for the tenderness, and turpentine, both internally and per rectum, for the tympanites, with asafoetida, and injections of solution of acidum carbolicum per vaginam. Urine had to be drawn again.

At 9 P. M.—Patient complains of less pain and tenderness; abdomen not so tense. Pulse and temperature about the same. Continued the same treatment.

April 7th, 8 A. M.—Pulse, 120; temperature, 104° F.; more pain and tenderness, and abdomen larger, from the accumulation of gases. The same treatment, also gr. x. of calomel, to move the bowels.

At 8:30 P. M.—Patient had two free movements from the bowels; otherwise much the same as she was in the morning. Ordered tinct. opii. deodorat in larger doses (gtts. xxx.) every two hours, and extracti veratri viridi, fl. gtts. v., to be repeated in two hours if she did not complain of nausea.

April 8th, 9 A. M.—Pulse, 108; temperature, $101\frac{3}{4}^{\circ}$ F. Tongue is clearing at the edges, and as a whole there was a change for the better.

For the rest of the details of this case I am indebted to Dr. Matson.

April 9th.—Patient continued improving.

April 10th.—Pulse, 100; temperature, 99° F. Everything going on nicely. Continued with the quinine three times a day, and the iron mixture as before. From this date patient kept on improving.

April 22d.—Dr. M. ceased his professional visits. Urine completely free from albumen.

April 25th.—Patient had a little relapse through imprudence in eating; pulse, 130; temperature, $103\frac{1}{2}^{\circ}$ F. This, however, subsided in a short time under proper treatment.

In looking over the above case it will be noticed that its treatment has been on general principles, so far as bodily health was concerned. As to the efficacy of chloral hydrate in this disease, I must say I was very much disappointed, especially after having read such vivid accounts of its power and virtue in such cases. This case, I think,

will confirm what I have said, and fully illustrate its inertness or failure in the present instance to mitigate or control the spasms, or to prolong the intervals between each attack, or to shorten their intensity, notwithstanding the doses were given as large and as frequently as in those cases reported elsewhere.

The foregoing case is interesting, owing to the diversity of opinion which still exists among many physicians, as to whether we ought to bleed in these cases. The present case will go to further corroborate the experience of those who are in favor of chloroform, while those who are opposed to this practice will readily admit the impropriety of having bled this patient in her anæmic condition. How true this was may be seen by the happy result and success met with in the case, thus fully sustaining us in our views of the case, and the course pursued therein. It cannot be denied, however, that bleeding in some cases is the treatment *par excellence* in puerperal convulsions, and especially is this the case in strong and plethoric patients. Even then I think we will find a most efficient agent in chloroform to control or shorten the spasms, as shown in the large number of cases which we read almost daily in medical literature. I omitted to state that in addition to treatment, the patient's strength, when she required it, was sustained by stimulants and plenty of beef-tea and milk.

In conclusion I wish to call attention to the dangers from the use of jaborandi. Not long since Dr. Jenkins, of New York, called attention to the objection to the hypodermic injection of jaborandi or its alkaloid pilocarpine in any case, and that is the flooding of the lungs by an excessive bronchial secretion.

Recently I was called unexpectedly to attend Mrs. C. again, in her seventh month of pregnancy, and whom I found having convulsions. After a while it was decided to have labor brought on, but the child was delivered still-born. The mother, however, continued suffering greatly after the birth of the child, and soon after became comatose. Jaborandi was tried first by the mouth, and afterward per rectum. Its peculiar physiological action did not show itself until three doses of it had been given. Simultaneously a profuse bronchorrhœa set in. The fluid was excessive, and the noise produced in respiration resembled the so-called "death-rattle." I may here state that previous to the administration of the fluid extract of jaborandi, chloroform inhalations were used, to mitigate or control the horrible spasms; also chloral hydrate and bromide of potassium, of each grs. xl. in solution, per rectum. The little urine obtained by the catheter was found on the application of heat and nitric acid to be almost solid with albumen. The case proved fatal. I think the case fully corroborates and confirms Dr. Jenkins' statement, and like him consider the above objection worthy of serious consideration.

DR. E. N. CHAPMAN: I feel a hesitancy in speaking, as, unlike those who have preceded me, I have not committed my views to paper; but still as I differ essentially on many points, I will attempt, as well as I may, to discuss the subject before the Society.

From all that I have heard at this meeting, it seems to be the general opinion that artificial delivery is the only certain resort in a severe case of eclampsia. Why? What has that to do with the kidneys? How will emptying the uterus carry off the urea from the blood? It is said that the abdominal muscles of a primipara strap the uterus so firmly against the spine that they induce congestion, and even inflammation of the kidneys, and that the pressure being taken off by delivery, the kidneys resume their functions and eliminate the urea. It is a little singular that an inflammation can be dispersed so easily and rapidly. Why is not the colon inflamed with the kidneys, as

the one suffers as much from pressure as the other? If this be the pathology in the case of a primipara before delivery, what explanation can be offered when the convulsions come on after the child has been born; and what explanation is there in the case of a multipara, seized either before or after labor? The fact is, the cause of puerperal convulsions must be sought elsewhere. The theory that has been held so long is not broad enough to cover the whole ground.

Then, again, the theory that the convulsions are due to urea in the blood is equally defective. Being, of late years, much in the habit of taking the specific gravity of the urine in all disorders of the kidneys. I have had several cases in which the solid constituents were reduced to a very low figure, and yet, though this state of things continued for months, no evidences of uræmic poisoning showed themselves. A lady, eleven years since, had, before and after being delivered of a dead child, urine of a sp. gr. of 1006. Her condition improved very slowly; and yet, at present, she enjoys prime health. A lady, suffering a kind of chronic colic, from scybala impacted in the sacks of the colon, had several turns of severe pain in the bladder, with the passage of blood in the urine. The urine was very scanty—about twelve ounces in the twenty-four hours—had a sp. gr. of only 1004. Why did she not have uræmic convulsions? A gentleman had albuminuria in a slight degree, and passed urine of a sp. gr. of 1011. Three days subsequent he had convulsions. These being held in check by chloroform, and ultimately subdued by hydragogue cathartics, saline diuretics, low diet, rest in bed, poultices over the loins, etc., the urine was found to still contain albumen, and to be of the same specific gravity. In two weeks the urine became normal, excepting that it lacked several degrees of the normal standard.

The true pathology of puerperal convulsions seems to be this: the uterine nerves, muscles, and areola tissue, are developed on conception taking place. The stimulus is felt by all the abdominal viscera. Hence the stomach is disturbed and the arteries stimulated. A pregnant woman has a regular, full pulse. Its volume is characteristic. Sometimes the pulse, from this stimulus, is small and quick, like that preceding tuberculosis. The walls of the uterus in a primipara do not expand readily to the influx of the unwonted tide of blood, and hence the uterine nerves, being imprisoned in a denser tissue, become greatly irritated. The whole sympathetic system is disturbed; the renal nerves more especially. Thus congestion of the kidneys arises, and occasions the albuminuria. At the same time, the other abdominal organs being congested, and the portal veins clogged, digestive secretion, assimilation, and excretion, are wholly perverted. The circulation of the blood, and the change in the tissues, is equally disordered. Just at this stage convulsions are prone to set in. The point of departure is the nerves of the uterus, and the several steps thereafter are the disturbance of the abdominal nerves, the perversion of all the various functions, the implication of the general nervous system, and, finally, an attack of convulsions.

There is a general similarity in all convulsive movements arising from irritation of organs chiefly under the domination of the sympathetic nerves. In a child, whose rapid growth demands a large supply of nourishment, the digestive organs are apt to be overtaxed, particularly when the food is not suited to its age. Then arises great irritability of the gastric nerves, and eventually a convulsive seizure, on some marked indiscretion in diet. The convulsions are similar to those of eclampsia, and are followed, when severe, by deep sleep.

A long and persistent abuse of the stomach may, in adults, induce an attack of epilepsy, as I witnessed in the case of a strong and healthy young man some twelve years since. He was given to the pleasure of the table, and gratified his appetite without stint, and was taken one Sunday afternoon, about an hour after dinner, with convul-

sions. These were very violent, and required four men to keep him on the bed; but they were more like those occurring in childhood. The second day after he had a characteristic epileptic attack. His memory thereafter was greatly impaired. He was treated with the bromide of potassium for the purpose of commanding the convulsions, and with a very low, unstimulating diet, for the purpose of removing the preternatural excitability of the gastric nerves. Three months subsequent he had another, but a slight attack, from inadvertency in eating. This was the last. He is now in perfect health, and has the full possession of his mental powers. He still adheres to a very simple diet. In epilepsy the convulsions are like those of eclampsia, as are also the coma and stertorous breathing. In the convulsions of albuminuria there is the same similarity. The male, from irritation of the renal nerves, has symptoms identical with those occurring in the female from irritation of the uterine nerves. There is this difference only: in pregnancy both the uterine and renal nerves are implicated. Therefore, I conclude that the congestion of the kidneys, the deficient elimination of the urea, the discharge of albumen, is secondary to the irritation set up in the uterine nerves, and the general disturbance of the sympathetic nervous system. A primipara should be under surveillance some two or three months before her confinement. Her food should not be highly nitrogenized, so as to throw great labor on the kidneys; her bowels should be thoroughly unloaded by a purgative now and then, as the bowels are semi-paralyzed by the nervous influence being absorbed by the uterus. Of course air, exercise, and other hygienic measures should be attended to. These precautions being neglected, she may become dull and sleepy, have a muddy countenance, and suffer neuralgic pains, particularly in the head. These are the first notes of warning. They all indicate an obstructed condition of the abdominal circulation. No attention being paid to these symptoms, soon there is violent neuralgic pain in the head, flashes of light, or dark motes before the eyes, noises in the ears, puffiness of the eyelids in the morning, and a scanty secretion of urine of a dark color. Eventually the feet swell, and albumen appears in the urine.

If a physician is fortunate enough to have the woman under his care during pregnancy, convulsions may almost always be forestalled. Everything must be done to prevent the strain upon the sympathetic nerves; those of the stomach, bowels, uterus, and kidneys especially. The food should be light and digestible; the alvine evacuations free and regular, so as to ensure an active circulation in the portal veins, and a proper secretion of all the glands. Otherwise, the blood becomes deteriorated, and the great nerve centres weakened. Should the crisis arrive and convulsions declare themselves, the original plan of treatment should, if it has previously been neglected, be followed—that is to say, procedure should be instituted to relieve the sympathetic nerves from the oppression under which they labor, and restore tone and functional activity to all the abdominal organs. All means failing, artificial delivery may relieve the uterine nerves, the original source of all the trouble, and give a chance to the other members of the same family to recover the power on which the integrity of the various functions, the sum of which constitutes life, are dependent. In this way, and not by specific remedies, puerperal convulsions may be prevented, and when they appear may be combated with a fair measure of success.

DR. T. N. DE BOWES took exception to the theories advanced by previous speakers as to the ganglionic theory of the origin of puerperal convulsions, for, when a nerve cell is destroyed, its action ceases.

DR. W. F. SANDFORD referred to the statistics of puerperal convulsions :

There were several points in Dr. Segur's paper which interested him in this regard. Dr. Segur seems to have quoted an unusual number of cases from high authorities where the results recorded by them are unusually favorable. One case of convulsion, occurring in from 200 to 500 confinements, is about the record of the books. If that is so, then the experience of the medical profession in Brooklyn seems to be against these statistics, for Dr. Segur estimates that there is one case of convulsion to every 71 confinements. If that is true, then the books must be changed, and we have a state of things in this city which is truly alarming. It is but just to say, however, that these figures are based upon an estimation only, and are not endorsed by Dr. Segur as correct. He says, this result "is so unexpected that we must hesitate to accept it." But the speaker did not think that the result is by any means correct, for the estimate, in the first place, was based on an unusual per cent. of recoveries—far greater, he believed, than the experience of the members of the Society, as given during this discussion, would indicate. And the proportion of cases of convulsions to births he believed to be not far from the record of the books, viz.: one in three to five hundred. He (Dr. Sandford) had taken the pains personally to look into the question. He had seen in his own practice one case of puerperal convulsions. The case occurred just at the delivery of the after-birth and without warning. The patient was a primipara and she recovered. The treatment consisted of large doses of elaterium, chloral and bromide and administration of chloroform. The urine showed large amount of albumen. As bearing upon the question of statistics, he took the stubs of the birth reports, which, added to the estimated cases of still births attended by him, numbered about 425 to 430; and he had had, among all these confinements, but one case of convulsions. The idea that we are to have one case out of every seventy is a mistake, in his estimation. He had seen, during his practice, five cases of convulsions, four occurring in the practice of his friends, and it seemed to him that that record gives a reason for overestimating the percentage of cases; *i. e.*, if he were asked to state how many cases he had seen, he would include not only his *own* case, but all other cases which he had ever seen. His neighbor, who may have seen some of the same cases, would be apt also to include them in his list, so that when the total number of cases was given, there would be an error, in that some of them may have been counted several times. This fact has a tendency to create a high percentage. He would recall one of the cases referred to. It occurred in his own family, on the second day after confinement, and was purely uræmic in character, without any premonitory symptoms. Two weeks previous to the confinement, albumen had been discovered in the urine. None four weeks previous. There were no signs of an attack just prior to the convulsions, but, upon raising her from the bed, there was a sudden and distinct convulsion, lasting less than five minutes, with no return. He was of the opinion that there should be a distinction between uræmic and puerperal convulsions proper. This was not a case of eclampsia, and yet it was a case in which albumen had something to do.

DR. L. S. PILCHER had been fortunate enough to have but one case in his own practice; but that case had been observed with care, and he had noted its various features. The record is as follows :

The patient had been of previous good general health, was 27 years of age, and had reached the eighth month of her fifth pregnancy. She had been unusually free from the usual discomforts of the pregnant state. Bowels and kidneys were apparently in good working condition. Slight œdema at the ankles had been noticed. During the evening of October 11, 1878, she suffered from intense supraorbital headache; at mid-

night she experienced a convulsion ; three others followed at intervals of little more than an hour, during the last of which she was first seen by the doctor. She had a pulse of 100 per minute. By the introduction of a catheter, but a tablespoonful of urine was obtained, which, when heated, coagulated into a solid mass. She was put under the influence of chloroform, elaterium, $\frac{1}{8}$ grain, administered, and ten ounces of blood removed by wet cups applied to lumbar region. Two hours later a second dose of elaterium was given. At end of three hours, after fourth convulsion, while still under influence of chloroform, a fifth convulsion, followed within a few minutes by a sixth. At this time, and throughout in the management of the case, the advantage of the advice of the President of the Society, Dr. Jewett, was enjoyed. During this sixth convulsion, at 9:15 A. M., October 12, a hypodermic of fluid extract of *veratrum viridis*, ℥ xii., was given, and the body of the patient kept enveloped in the hot steam generated by hot bricks wrapped in wet cloths, placed by the side of the body, under the blankets. The pulse was brought down to 44 within an hour after giving the *veratrum*. For three hours there was almost constant vomiting of copious bilious fluid, accompanied with much prostration, presumably the effect of the *veratrum*. It subsided spontaneously. The steam bath was kept up. At 3 P. M. the pulse was 50, temperature 99° F., bowels not yet moved. Drs. Skene and Emory joined Drs. Jewett and Pilcher in consultation. Induction of labor decided against. Calomel, gr. xx., followed by liquor sodæ tartratis, with hot lemonade administered. During the ensuing night there were no convulsions ; numerous copious movements of the bowels occurred ; the pulse was kept below 60 by *veratrum p. r. n.* ; the skin active ; about four ounces of urine were secured in addition to what passed when bowels moved.

Oct. 13.—The mind was clear, and she was free from headache. During this day and night ten movements of the bowels occurred ; there was a decided increase in the amount of urine passed, with great diminution in the amount of albumen. The treatment by the steam bath, saline diuretics and *veratrum* in sufficient quantity to keep the pulse below 60 was persevered in.

Oct. 14.—Third day. Improvement continues ; no convulsions ; steaming thrice daily substituted for continuous steam bath ; poultices to lumbar region ; in addition to saline diuretics, gave two drachms of the infusion of *digitalis* every three hours ; bowels move frequently.

Oct. 15.—Fourth day. 24 ounces urine have been collected during the past 24 hours ; general condition encouraging ; treatment unchanged ; hot milk as diet.

Oct. 16.—Fifth day. In the morning doing well. Though the treatment was ordered to be maintained with care, the attendants began to relax their watchfulness and to become remiss and irregular in the administration of remedies. At 5 P. M., four and a half days since the last convulsion, the seventh convulsion occurred, the pulse having risen to 92. Five minims of the fluid extract of *veratrum viridis* were at once given hypodermically ; an eighth of a grain of elaterium was administered and the steam bath resumed. Within two hours a copious movement of the bowels was secured, and the pulse had fallen to 44, attended with vomiting and continued gastric irritability for some hours.

Oct. 17.—Sixth day. During previous 24 hours, eleven ounces of urine were collected ; frequent movements of the bowels have occurred ; pulse has been kept below 60. During this day is very comfortable ; skin active.

Oct. 18.—Seventh day. 8 A. M. Twenty ounces of urine during past 24 hours ; proportion of albumen has been daily diminishing ; pulse, 56 ; skin moist ; no headache ; excellent night's rest, bringing much refreshment. 6 P. M. A slight convulsion ; an interval of over 48 hours since preceding one ; bowels have not moved during the day ; eight ounces of urine since 8 A. M. ; stomach irritable ; this convulsion was followed

by a condition of persistent hebetude, or semi-coma. Well-marked uterine contractions began to develop during the evening. Upon consultation with Dr. Skene, immediate delivery was agreed upon, and effected without difficulty, by dilating the quite dilatable os by the fingers, and extracting by the feet. This was accomplished at 10:40 P. M. The child, when delivered, made no effort to respire, though feeble pulsations of the heart were discernible. By persistent efforts at artificial respiration, maintained for *two hours*, it finally began to breathe, and lived ten hours. The uterus contracted well after the delivery of the child, but no improvement in the condition of the mother resulted. She died comatose at about 8 A. M., October 19, a little more than seven days after the first convulsion.

There are several points in this case, continued the speaker, which may bear upon the subjects which have been introduced for discussion. In the various phases which this prolonged case presented, most of the remedies, vaunted as palliative or curative, were used as the special conditions which demanded them arose: diaphoretics, diuretics, cathartics, blood letting, chloroform, veratrum viridis, digitalis, milk; all were of use, and each had ample time to exert its special effect. The skin was kept active for days; the special symptoms of uræmia disappeared, and a sufficient restoration of the renal functions was secured.

During several days the bowels were freely moved and thoroughly evacuated. There was a thorough cleansing of the whole alimentary canal, and whatever the cause might have been which produced the final catastrophe, it certainly was not an accumulation of irritating material in the bowels.

Finally, in accordance with the teaching of many obstetrical authorities, and the judgment of the gentlemen who saw the case, no attempt at delivery was made until spontaneous contractions of the uterus had developed. The delivery, when finally effected, at the very termination of the case, having thus become a *dernier ressort*, was without influence upon its course.

Reflection upon this case led Dr. Pilcher to look with favor upon the doctrine that, however complex may be the conditions which are involved in cases of puerperal eclampsia, the most important of all is the presence of the child within the uterus; that whatever the sequence of the causes which culminate in the convulsion, that here is their initial point, and that it is as irrational to leave the child in the uterus during the treatment of convulsions as it would be to leave a thorn rankling in a wound while a tetanus was overwhelming the body. It is true that the emptying of the uterus may not prevent an explosion of convulsions, but it seemed to him that a great advantage had been gained in the attempts to control them, and that until such emptying had been accomplished, imminent danger existed. In view of the contradictory opinions which are held, it was evident that there was much as to the pathology, the prophylaxis, and the treatment of puerperal convulsions yet to be settled. This, he believed, was to be chiefly secured by the study and analysis of carefully observed cases, rather than evolved from general and vague speculations, and he hoped that the experience of this Society, elicited in this discussion, would contribute something of value toward arriving at the desired end.

DR. T. M. ROCHESTER has had two cases which are pertinent to this discussion; one case of eclampsia. In the other case no convulsions occurred, although they were expected. In both these cases there were premonitory symptoms; in both the possibility of convulsions was recognized, and in both there was a certain amount of prophylactic treatment. The first case, in which there were no convulsions, occurred in the year 1876 or 1877, in Buffalo, N. Y. A young girl, unmarried; primipara. First saw her about three weeks before confinement. Previous to that time—about two or

three weeks—she had suffered from headache, disturbed vision and most intense œdema of the upper and lower extremities and face. When first seen she was ordered salines and other proper treatment. He did not see her again until a day after the confinement; but when she was suddenly confined nobody but a female attendant was present. There had been no convulsions; she felt entirely well, and there had been no bad symptoms at all during or after labor. When examined before labor the urine showed 50 per cent. albumen, which entirely disappeared a week or so after the child was delivered.

The second case was more interesting. It occurred in the spring of 1878 in the city of Brooklyn. Patient æt. 29, the labor being her third. Her regular physician was Dr. Speir; but it so happened that there was some mistake on the part of the messenger, and the case fell in Dr. Rochester's hands. When first seen the head was pressing on the perineum, and the child was delivered in a few moments. Dr. Speir got there shortly after the birth. Upon inquiry, Dr. Speir informed the speaker that the patient had had headache, œdema and other kindred symptoms; that he had tried the milk diet, rest in recumbent position, apollinaris water and other expedients. The child was born in the morning; she had had three or four movements of the bowels the preceding night, and had urinated freely. The case was seen again by Dr. R. at noon, when an anodyne was given, and about that time also she passed her urine. At 5 P. M. she was seized with convulsions, and chloroform was immediately administered. As soon as the convulsions had ceased the catheter was introduced, but only a few drachms of urine found. He then gave three drops of croton oil on the tongue, repeating the dose every twenty minutes until nine drops had been given, seemingly without effect. Six or eight hours after the bowels moved in response to enemata. He sent for Dr. Speir, who suggested nothing further, and thought she would probably die. Hydrated chloral and bromide of potash were given between the convulsions—15 grs. of each—and repeated frequently. The convulsions were excessively severe, always starting with a twitching of the left arm, and this she herself noticed, as she would say, "What is the matter with my arm?" After the first convulsion that phenomenon was used as a guide to the use of chloroform. She had five convulsions between 5 P. M. and 2 A. M.; when she had the last she gradually, from that time, improved; the action of the skin was kept up, the bowels were moved by enemata. Special treatment directed towards the kidneys was used, but the urine continued albuminous until ten days afterwards, gradually lessened in quantity and finally disappeared. She is now living and doing well. This case, however, is of interest, because Dr. Speir said he expected convulsions, and had used prophylactic treatment—milk diet, apollinaris water, etc. The result has been seen.

DR. J. C. SHAW: Convulsions are characterized by involuntary contractions of the muscles which are under the control of volition. They may be partial or generalized; fibrillary, tonic or clonic, or tonic and clonic combined. It is a symptom which may have its starting point in a variety of regions; in affections of the muscles, of the nerves, in lesions of the nervous centres, or in diseases located in other organs. In regard to the convulsions in puerperal disease, the fact is that these convulsions do not differ essentially from other convulsions, as far as the mechanism is concerned, and of the causes which bring about these convulsions there is much dispute. Convulsions are only a symptom, and a very common one, and the pathology and mechanism has to be studied in each individual case. In the convulsions which are brought about by reflex action, such as those which usually occur in puerperal cases, even here it is hard to say by what paths and in what manner the disturbance is carried to the medulla, or to the so-called convulsion centre of Nothnagel. It is believed that uræmia

or ammonia circulating in the blood from the non-performance of the proper function of the kidneys brings about these convulsions, or it may be, and I am disposed to believe, without much experience with puerperal convulsions, that in a number of these cases there is œdema of the lung, and which plays a large part in bringing about the low delirium, wandering, coma, convulsions and death. It is also believed by some that anæmia of the brain brings about the convulsions. All these views may be right, and I am disposed to think that each one of these factors comes into play in these cases; excrementitious material circulating in the blood, œdema of lungs, both bringing about anæmia of the cerebro-spinal centres; or, to speak more correctly, bringing about an arrest in the proper and healthy supply of nutrition (the blood) to the cerebro-spinal axis, and which is necessary to its normal function; hence we have the cephalalgia, low delirium, unconsciousness and convulsions. It is quite a question if the disturbance bringing about the convulsions starts in the cerebral cortex, thence down upon the pons and medulla; or if it be, as Nothnagel believes, that it begins in the pons and medulla first, simultaneously involving the vaso-motor centre and the convulsive centre, the consequent vaso-motor disturbance of the cerebral vessels causing the cerebral anæmia and the unconsciousness. All this, of course, is largely theoretical, and even this does not explain all, for it may be asked why does it occur that the convulsive action will begin in the muscles of one side of the face and the muscles of the neck, causing deviation of the head; then the extremities of one side become involved, and, although the convulsion may become generalized, yet the side on which it begins first be more affected all during the paroxysm than the other side? If this be due to a disturbance in the pons and medulla it must be greater on one side than the other, and if so, why is this apparent localization present? If the convulsion were due to an irritation starting at a definite point, we might then understand how it could be that the force of this irritation could make itself felt more decidedly on one side with which it may be in more direct communication, and be somewhat abated in force when it reaches the more distant parts; but here where we have a general condition apparently as to the cause of the convulsions, it is very difficult to understand why this localization should be. If I may be allowed to suggest that it would be of interest, and might eventually lead to results, to study the character of these convulsions somewhat from the point that Hughlings Jackson has studied epilepsies.

Observe in what muscles the convulsions begin, and what parts are successively involved, to which side the head and eyes deviate, etc. It has been shown recently by Landonzy, and from my own observations I believe him to be correct, that the head and eyes deviate to one side or the other, according as the lesion is an irritative or destructive one, and according to whether the lesion is in the cortex or pons.

DR. J. D. SULLIVAN had said on a previous occasion that he had had thirteen cases, and that one-half of them died. Since then, he remembered three or four recoveries. Two or three of these cases were simply seen in a dying condition and had no treatment. He would like to ask the experience of the Society with regard to the use of opium after convulsions. Two or three of the cases which he saw die after delivery; but about three years ago he commenced the use of opium in every case after delivery, and since then he had not lost a single case. As quick as he detects the convulsions after delivery he uses a hypodermic injection of morphia. His idea is that convulsions are dependent upon some irritation that still remains in the system after delivery of the child, and that opium overcomes that irritation or allays it, or at least appears to ward off the convulsions until the natural condition of affairs is restored. He had used opium in four or five cases with good effect.

DR. J. H. HUNT related a case *à propos* to Dr. Hutchins' remarks on non-uræmic signs of convulsions. Some three years ago he had a case, a primipara, who, just after delivery, was taken with a convulsion. The phases of this case did not strike him as being of the character of other cases which he had seen. He drew off some of the urine and examining it found it non-albuminous, and she had no symptoms of albuminuria; no anasarca; no œdema; and then the question was asked, whether this woman had ever had anything like epileptic convulsions. The family insisted that she had not, and he had nothing to do but put the case down as one of non-uræmic origin. However, in the course of the next fortnight he ascertained that the lady, who had recovered from the confinement, had had in the lodge room of an association of which she was a member an attack of what was called "fits." The solution of the problem was arrived at, and the case was properly noted as a case of epileptic convulsions during confinement.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

A regular stated meeting of the Medical Society of the County of Kings was held at No. 398 Fulton Street, on the evening of Tuesday, October 19th, 1880. The President, Dr. Charles Jewett, in the Chair, Dr. R. M. Wyckoff, Secretary, acting in that capacity.

The following named gentlemen registered:

Drs. Charles Jewett, R. M. Wyckoff, J. R. Vanderveer, H. G. Small, J. H. H. Burge, A. Campbell, V. Morse, A. Hutchins, E. A. Whaley, W. F. Sandford, J. D. Rushmore, J. J. Lamadrid, J. H. Hunt, A. Segur, A. L. Lowell, W. E. Conroy, E. N. Chapman, E. R. Squibb, S. J. Sørensen, T. M. Rochester, G. McNaughton, J. S. Wight, J. S. Prout, A. Mathewson, A. R. Matheson, H. N. Read, W. C. Otterson, B. S. Van Zile, W. Wallace, F. H. Stuart, E. Palmer, T. N. De Bowes, F. H. Barber, L. S. Pilcher, J. D. Sullivan, F. E. West, B. F. Westbrook, L. C. Gray, J. G. Wilbur, J. Walker, J. F. R. French, J. C. Hutchison, F. W. Wunderlich, R. Hesse, and G. Drury. Total, 45.

The meeting was called to order at 8:15, by the Chair, who stated that the reading of the minutes would be dispensed with, as they were not present.

The following named gentlemen were proposed for membership:

Dr. C. Eugene Gunther, 174 Clinton Street, by Dr. Westbrook; Dr. Edgar Elting, 658 Nostrand Avenue, by Dr. Pilcher; Dr. C. L. Hogeboom, 907 Bushwick Avenue, by Dr. Wyckoff; and Dr. C. E. Dunkley, No. 168 Street, by Dr. Westbrook.

There was no report from the Council.

THE CHAIR then announced the next order of business to be the adjourned discussion of Dr. Segur's paper, "The Prophylaxis of Puerperal Convulsions." Several written communications had been received, and these would be called for first. Among others, were papers by Drs. Alexander Hutchins, on the unannounced puerperal convulsions; and J. Ross Matheson, the latter being remarks in reference to nine cases of puerperal convulsions which had come under his personal observation; both being read by their authors.

THE SECRETARY, at the request of the Chair, read several short communications from Drs. C. L. Mitchell, H. B. White, Wm. G. Russell, C. W. Vrooman, and J. A. Brodie.

THE CHAIR asked whether any other papers had been prepared by members present; if so, an opportunity would be offered for reading them.

Drs. William Wallace, J. H. Hobart Burge, and Julio J. Lamadrid, in response to this invitation, read short papers.

The meeting was then thrown open to a general discussion, and Drs. E. N. Chapman, L. C. Gray, T. N. De Bowes, L. S. Pilcher, W. F. Sandford, T. M. Rochester, F. H. Stuart, J. D. Sullivan, and J. H. Hunt, joined in the debate.

DR. STUART, in the course of his remarks suggested the appointment of a committee, whose duty it be to collect statistics from the members of the Society covering all cases of puerperal convulsions which have occurred in their practice, together with such facts relating thereto as can be secured.

The suggestion was afterwards made a resolution, which being duly seconded, was unanimously adopted. The Chair being so instructed, appointed the following gentlemen a committee under the resolution, namely: Drs. F. H. Stuart, A. Segur and W. F. Sandford.

THE CHAIR requested all members who had presented written communications to leave them with the Secretary.

DR. THOMAS R. FRENCH, of the Library Committee, presented an extended report showing the origin, progress and results of their labors.

MR. PRESIDENT AND GENTLEMEN:

The Library Committee beg leave to submit to the Society the following report:

It having been long felt by the members of the Society that a more extensive library was needed, as well as some permanent provision made for the acquisition of books, beyond the occasional donations by authors and deceased members, a discussion of the subject *in extenso* was entered into by the Society at the last Annual Meeting, at which it was ordered by the Society that the Council appoint a committee to solicit subscriptions for a Library Fund.

The Council appointed a committee of thirteen members, for the fulfillment of the purpose of the Society, known as the Library Committee. The Committee held their first meeting early in February at the Library rooms, and proceeded to organize for work by selecting Dr. French as Chairman and Dr. Rand as Secretary. The city was then divided into districts, and each member allotted a certain district containing a given number of physicians, members of this Society. Every member of the Society was in this way to be seen, personally, by a member of the Committee, the object of the Committee explained and a subscription solicited. It was resolved to ask no specific sum, nor any large amount from any individual—the idea being, rather, to have *all* the members of the Society give *something*, no matter how small an amount, as it was believed that the interest in the work and the sense of personal proprietorship in the Library would be enhanced by this method. The work of canvassing was immediately entered upon, and was continued through one month, three meetings of the Committee being held to present subscriptions, report progress, etc. The final meeting was held on the 16th of February, and the results compared. The amount of work done may be inferred when it is stated that, with but two or three exceptions, every member, of the four hundred physicians composing this body, had been seen some two or three times, and a thorough discussion of the Library scheme had been entered into with each gentleman, views exchanged, changes suggested and plans discussed. The Committee reported that, with few exceptions, the scheme met with the greatest favor and even enthusiasm. Nearly every member subscribed something, ranging in amount from twenty-five cents to ten dollars, and the final collections showed the sum of \$514 61 for the Library Fund. When it is remembered that the yearly dues were increased to five dollars this year, the Committee feel that they may congratulate the Society on the handsome sum obtained, and feel sure that the Society will not suffer a work, which

has begun so auspiciously, and is evidently so much in favor with the members, to be abandoned in the future, but will make some provision for a permanent *Library Fund*. Not only has the subscription of so large a sum been a matter of felicitation to the Committee, but the thorough canvassing of the subject for obtaining a good library among the members of the body induces the belief in the minds of the Committee that in zeal for professional knowledge, desire for self-culture, determination to keep abreast with advancing scientific discovery, and in general devotion to the honor and advancement of medicine, this body is second to none of like character in the country; and they feel convinced that the success met with this year is but an earnest of what will be done in the future; and that, when the Society has a home of its own, it will have the nucleus of a Library which will in time be a complete one, and invaluable to the profession in Brooklyn. At the same meeting, when the whole amount of donations had been paid in, the Committee appointed a Sub-Committee of three, consisting of Drs. Rand, Hunt and French, with the Librarian *ex-officio*, known as the Library Sub-Committee, whose duty it should be to select and purchase books to the amount of the five hundred and fourteen dollars, receive, stamp and arrange them, and make the best arrangement possible, under the circumstances, for their custody and care.

The Library Sub-Committee met accordingly on the 19th of March and organized for work. The selection of the books was a work of no small responsibility. Postal cards were sent to every member of the Society, and each one was invited to send a list of books which would be most acceptable to him, individually, or else give a general opinion as to the kind of books he believed to be most needed. Only a few replies were received, however; the majority giving it to be understood that they were willing to trust the matter of selection to the Committee. The published lists of some half dozen of the principal dealers in medical books were then obtained, and the selections made, a certain number of books allotted to each of the different departments of medicine, viz.: Anatomy and Physiology; Histology, normal and pathological; Pathology; Hygiene; Practice of Medicine and Surgery, with its various specialties; Ophthalmology; Otology; Laryngology; Dermatology; Diseases of the Mind and Nervous System; Diseases of Children; Gynæcology; Chemistry and Medical Jurisprudence; Electricity and Obstetrics.

The aim of the Committee has been to make the Library eminently practical and useful to the great body of the Society, and yet to secure some books really rare and expensive, which few private libraries contain, which are of great use, invaluable in fact, in special departments. Cobbold's extensive and learned work on Entozoa, and Jonathan Hutchinson's magnificent Illustrations of Clinical Surgery may be instanced among the latter category.

In the selection of works on special subjects the opinions of gentlemen engaged in the practice of the different specialties were obtained.

The whole list comprises 157 volumes, to the value of \$473.94. Some few of the books were purchased privately by members of the Committee, and the rest were offered to the lowest bidder by competition. Bids were received from seven different firms, and, on examination, Presley Blakiston, of Philadelphia, was found to have offered the best terms. He was accordingly awarded the contract, and shipped the first installment of books to the Committee on June 30th.

The work of unpacking, cutting, stamping and arranging the books has at length been finished—twelve meetings being found necessary to accomplish the work—and the Committee have now the satisfaction of inviting an inspection of the new Library. Appended to this report will be found the list of works purchased.

In conclusion, the Committee, believing firmly in the popularity and usefulness of

the Library scheme, venture to hope that the Society will go on in the good work so well begun, and suggest that the plan of voluntary subscriptions be continued in the coming year; no special tax to be levied, no dues collected, simply the amount which each one feels disposed to give, no matter how small a sum, in the furtherance of this object of such great and obvious advantage to us all. Our large membership shows that a small sum, twenty-five or fifty cents, from each one a year will, in ten or a dozen years, give us a library of magnificent proportions, and a pride to our profession.

There have been also donated by members of this Society the following books:

Maclagan on the Germ Theory of Disease.

Billroth's Surgical Pathology.

Anstie on Stimulants and Narcotics.

Althaus on Electricity.

Foster, Klein & Sanderson's Handbook for the Physiological Laboratory.

Frey on the Microscope.

Stellway on the Eye.

Brooklyn, 19th October, 1880.

T. R. FRENCH, M.D., Ch'n,	H. N. READ, M.D.,
G. W. BAKER, M.D.,	J. E. RICHARDSON, M.D.,
L. C. GRAY, M.D.,	F. W. ROCKWELL, M.D.,
J. H. HUNT, M.D.,	W. G. RUSSELL, M.D.,
A. HUTCHINS, M.D.,	W. WALLACE, M.D.,
A. R. MATHESON, M.D.,	G. WIEBER, M.D.,
H. W. RAND, M.D.,	H. F. WILLIAMS, M.D.,

Committee on Library.

LIST OF MEDICAL and SURGICAL WORKS

PURCHASED BY THE

Library Committee of the Medical Society of the County of Kings, 1880.

ANATOMY AND PHYSIOLOGY.

Bock's Atlas of Human Anatomy.

Braune's Atlas of Topographical Anatomy.

Herman's Physiology.

Humphrey. The Human Skeleton.

Klein's Anatomy of the Lymphatic System, 2 vols.

BRAIN, NERVOUS SYSTEM AND INSANITY.

Anstie on Neuralgia.

Charcot's Diseases of the Brain.

Hamilton on Nervous Diseases.

Liveing on Megrim and Sick Headache.

Mitchell on the Nerves.

Tuke's Insanity.

CANCER.

Marsden on Cancer.

CHEMISTRY.

Roscoe and Schorlemmer's Chemistry, 3 vols.

CHEST (DISEASES OF).

Balfour on the Heart.	Flint's Percussion and Auscultation.
Bennett's Pulmonary Consumption.	Fothergill. The Heart.
Buhl's Inflammation of the Lungs.	Greenhow's Chronic Bronchitis.
Flint's Phthisis.	Hayden on the Heart.
Flint on Chest and Lungs.	Powell on Consumption.
Williams. Pulmonary Consumption.	

CHILDREN (DISEASES OF).

Meigs and Pepper on Children.	Smith. Wasting Diseases of Child'n.
Smith. Diseases of Infancy and Childhood.	Steiner's Compendium of Children's Diseases.
Vogel on Children.	

DICTIONARIES.

Cutter's Dictionary of German Terms used in Medicine.
Dunglison's Medical Dictionary.

ELECTRICITY.

Beard and Rockwell on Medical and Surgical Electricity.

EAR.

Burnett on the Ear.

KIDNEYS, LIVER, ABDOMEN.

Dickinson on Diabetes.	Habershon on Alimentary Canal.
Dickinson on Albuminuria.	Johnston on Bright's Disease.
Murchison on Diseases of the Liver.	

MATERIA MEDICA, THERAPEUTICS AND TOXICOLOGY.

Bartholow's Materia Medica and Therapeutics.	Wood's Materia Medica, Therapeutics and Toxicology.
Ringer's Therapeutics.	

MISCELLANEOUS.

Beale on Protoplasm.	Mitchell's Fat and Blood.
Buck's Hygiene and Public Health.	Taylor's Medical Jurisprudence, 2 vols.
Chamber's on Diet.	Wormley on Micro-Chemistry of Poisons.
Cobbold on Entozoa.	
Fox's Artificial Tubercle.	
Legg on Hæmophilia.	

OBSTETRICS.

Barnes' Obstetric Operations.	Playfair's Midwifery.
Cazeaux's Midwifery.	Simpson's Obstetric Memoirs.

PATHOLOGY AND HISTOLOGY.

Cornil and Ranvier's Pathological Histology.	Paget's Surgical Pathology.
Creighton's Physiology and Pathology of the Breast.	Rindfleisch's Pathological Histology.
Green's Pathology and Morbid Anatomy.	Virchow's Cellular Pathology.
	Wilks and Moxen's Pathological Anatomy.

PRACTICE OF MEDICINE.

Aitken. Practice of Medicine, 2 vols.	Reynold's System of Medicine, 3 vols.
Da Costa's Medical Diagnosis.	
Flint's Clinical Medicine.	Tanner's Practice of Medicine, 2 vols.
Niemeyer's Practical Medicine, 2 vols.	Trousseau's Clinical Medicine, 2 vols.

SKIN DISEASES.

Fox's Skin Diseases.	Piffard's Skin Diseases.
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SURGERY.

Adams on Club Foot.	Holmes' System of Surgery, 4 vols.
Adams on Finger Contractions.	Hutchinson's Illustrations of Clinical Surgery, 1 vol. and 3 parts.
Allingham on Diseases of Rectum.	Markoe on Bones.
Ashhurst's Practice of Surgery.	Mason's Surgery of the Face.
Broadhurst's Orthopædic Surgery.	Sayre on Spinal Disease and Curvature.
Buck's Compendium of Surgery.	Surgery in the Pennsylvania Hospital.
Clark on Diseases of the Tongue.	Thomas on Intestinal Obstructions.
Dieulafoy's Pneumatic Aspiration.	
Gant's Practice of Surgery, 2 vols.	
Hamilton's Surgery.	

Wyeth's Surgical Anatomy.

THROAT AND NOSE.

Brown on Diseases of the Throat.	Mackenzie on Diphtheria.
Cohen on the Throat and Nasal Passages.	Thornton's Tracheotomy.
Mackenzie on Laryngeal Growths.	Watson on the Nose.
	Wyman on Autumnal Catarrh.

URINARY ORGANS.

Otis on Stricture of the Male Urethra.	Van Buren and Keyes' Genito-Urinary Diseases.
Roberts on Urinary and Renal Disease.	

VENEREAL DISEASES.

Bartholow on Spermatorrhœa.	Keyes on Syphilis.
Bumstead on Venereal Diseases.	Lewin on Syphilis.

WOMEN (DISEASES OF).

Barker's Puerperal Diseases.	Ercolani on the Utricular Glands.
Barnes on Diseases of Women.	Goodell's Lessons in Gynecology.
Emmet's Principles and Practice of Gynecology.	Sims' Uterine Surgery.
	Thomas' Diseases of Women.

WOOD'S LIBRARY FOR 1879.

Hilton on Rest and Pain.	Phillips' Materia Medica and Therapeutics.
Wardell on Diseases of the Intestines and Peritoneum.	Frerichs on Disease of the Liver, 3 vols.
Tait on Diseases of Women.	Rosenthal on Diseases of the Nervous System, 2 vols.
Routh on Infant Feeding.	
Ellis on Diseases of Children.	Clark's Manual of Surgery.

WOOD'S LIBRARY FOR 1880.

Keyes on Venereal Diseases.	Putzel on Functional Nervous Diseases.
Poulet on Foreign Bodies in Surgery, 2 vols.	Mackenzie on Throat and Nose, 1st vol.
Gutmann's Physical Diagnosis.	Buck on Diseases of the Ear.
Trousseau's Therapeutics, 3 vols.	Mundé on Minor Surgical Gynecology.
Savage on Anatomy and Surgery of the Female Pelvic Organs.	

HEALTH PRIMERS.

Hearing, and How to Keep It.	The Winter and its Dangers.
Long Life, and How to Reach It.	The Mouth and the Teeth.
The Summer and its Diseases.	Our Homes.
Eyesight, and How to Care for It.	Brain Work and Overwork.
The Throat and the Voice.	Sea-Air and Sea-Bathing.

THE CHAIR in rising to suggest the adoption of the report, said the Society was to be congratulated on the splendid results of this movement. It certainly, as a Society, is under peculiar obligations to each and all the members of the committee, but especially to Dr. French, who was the originator of the plan which he and his associates have so finely executed.

The report was then adopted.

On motion, it was resolved that the thanks of the Medical Society of the County of Kings be and they are hereby extended to Dr. French and his associates of the Library Committee for the able, thorough and satisfactory manner in which they have performed their labors.

Dr. WYCKOFF trusted that this action would not be considered as a discharge of the committee; and, if necessary, he would move that they be considered not discharged, but still in the active performance of their duty as a Library Committee.

The motion was duly seconded, and unanimously adopted.

THE PRESIDENT declared the following gentlemen to be members of the Society, namely: Geo. A. Schwartzenberg, John Willard, Geo. W. Wells and Lucian T. Bell.

DR. BURGE, rising to a question of privilege, said that his name had been inadvertently inserted in the minutes of the discussion on vaccination at the last meeting of the Society; but as he was not present, he had taken no part in that discussion. He regretted, however, that he was not there; and if it comported with the pleasure of the Society, he would be glad, at the next meeting, to take exception to some positions which other members had assumed, and to declare some views of his own which he hoped would provoke a new discussion upon some points not alluded to on the former occasion, including the question propounded by the President in reference to the non-inoculability of tertiary syphilis.

THE CHAIR said he would be glad to incorporate the proposed paper in the programme for the next meeting. He also, in this connection, read the titles of the papers to be read at the next session of the Society, which will be found in the usual place in THE PROCEEDINGS.

The Committee charged with the duty of preparing a suitable minute relative to the death of the late Dr. Thorne, did not respond to the call of the Chair; and the matter was therefore laid over until the next meeting.

There being no further business, the Society, on motion, adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρωποῖσι, κακῶν θελκτῆρ' ὀδυνῶν.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—MEDICAL EDUCATION AND PRACTICE IN ALL PARTS OF THE WORLD, by Dr. Hardwicke, of Sheffield, England (to be had of Mr. O'Connor, 68 & 70 Court St., Brooklyn), should be studied by all who are interested in medical progress.

Commencing with Europe, it gives the requirements for obtaining medical degrees from all sources in each country, and informs us under what conditions medicine may be practiced in each. Europe, Asia, Africa, North America, South and Central America, Australia, the East and West Indian Islands, and finally Oceanica, are considered in the above order.

Criticism, like charity, should begin at home, and so we find our author taking sharp account of the shortcomings of Great Britain and Ireland, of which he says:

"There is scarcely a European country where the legal conditions for practice are in such an unsatisfactory state; scarcely anywhere in Europe can a license to practice be obtained upon such easy conditions, and nowhere are the laws for preventing illegal practice so easily and so often evaded and set at naught. In France, Austro-Hungary, Italy, Russia and Sweden, all the examining bodies are regulated by the Government and have exactly the same requirements for the medical license; in Germany, Denmark, Norway, Holland, Spain, Switzerland, Portugal, Greece, Servia and Roumania, there is, in each, but one State licensing board; * * whilst in Great Britain and Ireland there are nineteen different licensing bodies, each granting several qualifications, the requirements of which are not in two cases alike, and which, together, make the number of diplomas for legal practice amount to fifty-five." Introduction, page 3.

In the body of the work (p. 10) we are informed that,

"Besides these examining bodies there is one other source from which a medical degree can be obtained, viz.: from His Grace the Lord Archbishop of Canterbury, who has the power to grant what is called the Doctorate of Medicine of Lambeth." He adds that such a degree "has recently been conferred upon a gentleman residing in Wales."

Particulars are fully given in regard to each German University degree,

and also as to the State examinations for license to practice, which are thorough and practical.

Much space is devoted to the regular and irregular medical schools of this country, and in many places copious extracts are made from their circulars. Errors and omissions have been noticed here; for, example, nothing is said of the Homeopathic School of the University of Michigan, and the Boston University School of Medicine, Homeopathic, is also omitted. But all of our *regular* schools seem to be given, and we are thus enabled to compare them with those of foreign countries.

After criticising the laws of Pennsylvania regulating the practice of medicine, he shows the friendly spirit in which he writes by adding:

“Fortunately for the Pennsylvanians, the regular medical institutions, granting degrees in medicine, are now as respectable as the law is bad.”—p. 182.

He rejoices exceedingly in the suppression of the Philadelphia bogus diploma mills.

In spite of certain minor shortcomings, the book is entitled to our hearty commendation.

—AN aged miser, feeling unwell, but grudging to pay a doctor's fee, sees with pleasure a medical gentleman with whom he is acquainted advancing towards him on the sidewalk.

“How are you?” exclaims the doctor, meeting him.

“Well, I don't feel very well, doctor,” says the miser; “in fact, I am quite out of sorts. I have no appetite, my tongue is coated, I have pains in the back and in the head,” and so on.

“Hum!” says the man of science; “that's pretty bad!”

“What would you advise me to do, doctor?” says the avaricious one, artfully.

“To consult a doctor, by all means,” says the doctor, walking away.

—HIGH ALTITUDE FOR CONSUMPTION.—It is stated that from a very remote period of time, it has been the custom among the Peruvians, before the conquest by the Spaniard, to send consumptives from the coast-line to heights of 8,000 to 10,000 feet on the Andes, often with signal benefit to the patients.

—MALARIA PREVALENT.—In New England malarial disease, especially intermittent fever, is unusually rife. The Massachusetts State Board of Health, having been led to look into the question of its alleged increase, is in the possession of facts which abundantly prove that it is not an imaginary increase, or a mere fashion to speak of the prevalent malaria. Near Providence, R. I., ague has been observed of a type almost as severe as that which has its home in our Southern States.

—BROMIDROSIS.—Dr. Ortega (*Le Praticien*) advocates the use of a solution of chloral in this affection. A patient of his, a strong man working in an ice manufactory, suffered from it in an extreme degree, so much so that his fellow workmen would not work by his side. The epidermis of the soles of the feet was white, as if macerated; there were small ulcerations in the furrows, and also around the nails. The odor was overpowering. Dr. Ortega prescribed baths of a solution of chloral (1 in 50) and wrapping the feet in a cloth dipped in a similar solution. Two days after the smell had disappeared. Six days later, the treatment being continued, the ulcerations were less moist and covered with a layer of epidermis.—*Press and Circular*.

—MALPRACTICE SUITS will not occur here so frequently as formerly. Our judges are falling into the habit of summarily dismissing causes, brought against medical men, if the plaintiff cannot show a good and substantial ground of grievance. The cases that have recently been taken up “on spec.” have fared badly.

—THREE LAWS are said to have been enacted in one of the Eastern States: 1. A law prohibiting the practice of medicine without a course of study including dissection. 2. A law prohibiting the dissection of any but criminals. 3. A law abolishing capital punishment.—*Pacific Med. and Surg. Jour.*, Oct.

—HEGAR'S SOLUTION.—In our October issue we gave the formula for Wickersheimer's solution. It has been reported by some as being an inefficient preservative of subjects. The *Ohio Medical Recorder* advises Hegar's solution to be used instead of the former where a non-poisonous preservative is required; it has a pleasant odor, and exterminates moths and vermin. Hegar's solution is composed of salicylic acid, 20 parts; boracic acid, 25; potassium carbonate, 5; dissolve in hot water 500 parts; glycerine, 200 parts; add oil of cinnamon and cloves, 15 parts each; dissolved in alcohol, 500 parts.

—MR. GEORGE DUNCAN has just returned from a three months' visit in Europe, and will enter at once on his rounds among the members of the Society who have put off till his return the payment of their annual dues.

SEE PAGE L.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P. M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The November meeting will be held on the 16th, at which the following papers will be presented:

Diseases of the Glands of the Female Urethra, by Dr. A. J. C. Skene.

A Case of Chronic Empyema, with extensive Amyloid Degeneration. Recovery. Remarks on the Subject of Amyloid Degeneration, by Drs. P. H. Kretschmar and B. F. Westbrook.

Brief Review of the Society's late Discussion on Vaccination, with Remarks on Humanized Virus, and on Non-inoculability of Tertiary Syphilis, by Dr. J. H. H. Burge.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President</i>	C. JEWETT, M.D., 307 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
<i>Secretary</i>	R. M. WYCKOFF, M.D., 532 Clinton Ave.
<i>Assistant-Secretary</i>	J. H. HUNT, M.D., 419 Hart St.
<i>Treasurer</i>	J. R. VANDERVEER, M.D., 301 Carlton Ave.
<i>Librarian</i>	A. HUTCHINS, M.D., 796 De Kalb Ave.

CENSORS.

B. A. Segur, M.D., 281 Henry St.	F. W. Rockwell, M.D., 6 Lafayette Ave.
E. R. Squibb, M.D., 36 Doughty St.	L. S. Pilcher, M.D., 4 Monroe St.
J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK. (1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI, Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Dr. R. M. Buell
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

PROCEEDINGS
OF THE
MEDICAL SOCIETY OF THE COUNTY OF KINGS.

VOL. V.

DECEMBER, 1880.

No. 10.

DISEASES OF THE GLANDS OF THE FEMALE
URETHRA.

BY A. J. C. SKENE, M.D.

Last spring I called attention to the anatomy of two important glands of the female urethra, and made some contributions to their pathology. Since that time I have had opportunities for extending my observations, and I now offer the result of my investigations to this Society. For the benefit of those who may have overlooked the original paper in the *Obstetrical Journal*, and with the view of making what follows more clear, I will quote briefly the description of the anatomy of these glands.

Upon each side near the floor of the female urethra there are two tubules large enough to admit a No. 1 probe of the French scale. They extend from the meatus urinarius upwards, from three-eighths to three-quarters of an inch, parallel with the long axis of the urethra. They are located beneath the mucous membrane in the muscular walls of the urethra. The mouths of these tubules are found upon the free surface of the mucous membrane of the urethra within the labia of the meatus urinarius.

The location of the openings is subject to slight variation, according to the condition and form of the meatus. In some subjects, especially

the young and very aged, and in those in whom the meatus is small and does not project above the plane of the vestibule, the orifices are found about an eighth of an inch within the outer border of the meatus. When the mucous membrane is thickened, relaxed and slightly prolapsed, or when the meatus is everted, conditions not uncommon among those who have borne children, the openings are exposed to view upon each side of the entrance to the urethra. The upper ends of the tubules terminate in a number of divisions which branch off into the muscular walls of the urethra. These branches can be demonstrated by injecting the tubules with mercury and then laying them open.

I have called them glands because they differ in size and structure from the simple follicles found in abundance in this portion of the mucous membrane. When they were first discovered I presumed that they were mucous follicles which were accidentally of unusual size in the subject examined, but having investigated more than two hundred of them in as many different subjects, in which they were present and uniform in size and location, I became satisfied that they were worthy of a separate place in descriptive anatomy. This attention they have not heretofore received, nor have the diseases to which they are subject been referred to by pathologists. At least I have been unable to find any notice of them in the standard text-books on anatomy and gynecology in English, French and German. It is easy to understand why these insignificant structures should have been overlooked by anatomists, or, if noticed at all, to be classed with the ordinary mucous follicles. It is only when their pathology is understood that their real importance becomes apparent.

The diseases of these glands to which I invite your attention at the present time are :

First.—Subacute inflammation or catarrh.

Second.—Gonorrheal inflammation and its results or products.

Third.—Inflammation following vulvitis such as occurs in strumous children.

Fourth.—Tuberculosis.

The first affection named in the classification is a mild form of catarrhal inflammation which occurs in connection with subacute vaginitis such as we find accompanying ordinary uterine disease or following parturition. This condition gives the patient very little, if any, inconvenience, and readily passes unnoticed by the gynecologist unless especially looked for. The mouths of the ducts are slightly enlarged, and sometimes surrounded by a very narrow areola of a bright-red color. By pressure upon the urethra from behind forward they discharge a white serous fluid. The cases which have come under my observation were detected while examining for other diseases, and none of them were attended with

any marked symptoms. In some of them the inflammation disappeared without treatment. In others it continued without showing any tendency to increase in severity or lead to important changes of structure. It is quite possible that a non-specific vaginitis might induce a high grade of inflammation in these glands with all the pathological changes to be described hereafter, but up to the present time I have not observed any evidence that such is the case.

Gonorrheal inflammation of these glands is of the chronic purulent variety, and in time extends from the mucous membrane of the ducts to the surrounding tissues. It does not usually attract attention until the vaginitis and urethritis has subsided.

The lesions presented differ according to the length of time which the disease has existed. When examined early there is slight swelling of the lower portion of the urethra. The mouths of the ducts are larger than normal, and the tissues around them are congested. There is tenderness to the touch, and pressure upon the urethra from above downwards causes a free purulent discharge. Sometimes it is necessary to separate the labia of the meatus in order to see the orifices of the ducts. In cases of longer standing the mouths of the ducts are brought into view by a slight prolapsus and eversion of the mucous membrane caused by swelling. The mucous membrane in the neighborhood of the ducts becomes thickened by proliferation of the areolar tissue and epithelium, presenting an irregular papillomatous appearance of a deep red color, upon the inner sides of which the orifices of the ducts appear like minute ulcers, of a yellowish gray color. The lower third of the urethra is generally thickened and indurated. The general appearance of the parts is quite like caruncle or papilloma of the meatus. In fact, inflammation of these glands has been mistaken for caruncle, at least it has been my misfortune in the past to confound the two affections, and I cannot see how others could have made a differential diagnosis, if guided by the current literature upon the subject. In a large proportion of the cases of this disease I have observed that upon the inner side of the labia minora, which rests upon the meatus, there are patches of inflammation which are caused and kept up by the purulent discharge from the glands. These circumscribed patches of inflammation sometimes extend downwards on each side of the introitus, and occasionally involve the carunculæ myrtiformes. This gives rise to much tenderness, which simulates vaginismus. The chief symptoms are extreme tenderness to the touch, great discomfort in sitting and walking, occasional sharp stinging pain, and a continual sense of heat in the parts. There is painful urination in some cases, and in others there is not. In some of the most marked cases that I have seen, this symptom was entirely absent, while in less

severe forms it has been present. That peculiar difference in the history of cases I have attributed to the fact that in the well-developed forms of the disease there is considerable eversion of the lower portion of the urethra, which throws the diseased and tender portion outwards, and thereby prevents the urine from coming in contact with the irritable surfaces. Occasionally there is frequent urination, due most likely to sympathetic irritation of the bladder. The symptom which is always present, in varying degrees of severity, is tenderness. The diagnosis and treatment may be left unnoticed until the other two affections of these glands have been described.

Purulent vulvitis, which occurs in children, especially those of scrofulous diathesis, occasionally extends to these glands. When such an extension of the disease occurs, it adds to its well-known rebelliousness to treatment. The original inflammation of the vulva may be relieved, but if the glands are involved, the purulent discharge from them will soon light up the disease of the external parts. From my own observations I believe that these glands rarely become involved, but when they do there is little possibility of curing the affection of the vulva until the glands are first successfully treated. There is really nothing peculiar in the clinical history of this form of disease, except its ethology, and therefore I need not dwell longer upon it, further than to say that I have seen a case of this kind, which had resisted treatment for a long time, but promptly recovered after the inflammation of the glands was detected and treated.

Tuberculosis, or tubercular inflammation of these glands, is an affection to be distinguished from the other forms of disease already considered. It occurs only in those who are of the tubercular diathesis, and may appear as a primary affection, or be developed during the progress of tubercular disease of other organs of the body. When the disease is first established it presents the same pathological appearances as have been described under the head of gonorrheal inflammation. There is, apparently, the same purulent discharge, with redness and proliferation around the mouths of the ducts, giving the peculiar caruncular or papillomatous appearance. The only peculiar characteristics of this affection that have been observed up to the present time, are the accumulation of caseous material in the tubules, and ulceration, which occur in more advanced stages of the disease.

The ulceration takes place in the newly formed tissue in the walls and around the mouths of the tubules. These caseous concretions and ulcerations are not found in all cases. Indeed, they are rare.

There is generally urethral inflammation accompanying this condition of the glands. It sometimes begins simultaneously with the disease of

the glands, and when it does not it follows soon after. In time the bladder becomes affected, and also the kidneys. At whatever point the disease commences it increases in severity, and extends until the whole of the urinary organs are involved, unless the patient succumbs before it has completed its progress. In some cases there are polypi and papillary growths of small size found along the urethra. These, I believe, originate in inflammation of mucous follicles and papillæ of the mucous membrane.

The symptoms presented in this form of disease are the same as those found in the other forms already described. From this it will be observed that the physical appearance and the symptoms are insufficient to establish a diagnosis. When there are ulcerations and caseous deposits the disease may be strongly suspected of being tubercular. Still, there is room for doubt until we find tuberculosis of other organs. This either precedes or soon follows the appearance of the disease of the glands.

In all the cases which have come under my observation the lungs were either tubercular when the patients were first seen, or became so soon after.

This affection is a source of great annoyance and suffering, and no doubt hastens the progress of the pulmonary disease with which it is generally accompanied. It has also another very important significance in the fact that it indicates the commencement of general tuberculosis of the urinary organs. The diagnosis of tubercular cystitis and urethritis has always been exceedingly difficult in the early stages of the disease. Indeed, it has been deemed impossible by most authors to distinguish ordinary cystitis from the tubercular form until the disease became developed in other organs of the body. Now that tuberculosis of these glands is understood, a valuable aid to diagnosis has been gained. Whenever an inflammation of these glands is found that cannot be traced to a former gonorrhea or vulvitis, it is almost sure to be tubercular, and the diagnosis is placed beyond doubt if the patient has the tubercular diathesis.

I am greatly indebted to Dr. Terrillon, of Paris, for some very valuable information upon the relations of disease of these glands to tuberculosis. In the *Progrès Médical* for this year he published a very elaborate article entitled "Polypoid Excrescences of the Female Urethra, Symptomatic of Tuberculosis of the Urinary Organs," which is full of original observations of inestimable value. In comparing his observations with my own I am fully satisfied that he has mistaken tubercular inflammation, and its products, of these glands, for excrescences, in some of his cases at least. Without being aware of the presence of these glands, it is perfectly natural that he should class those vascular developments found at

the meatus urinarius among the ordinary neoplasms of the urethra, just as all others have done in the past. There is every reason for believing that the excrescences which Dr. Terrillon refers to differ in their essential pathology from the ordinary polypoid growths, usually called *carunculæ*, which are found in the urethra, and are not associated with tuberculosis. And as the history of his cases coincide with the history of the cases of tuberculosis of these glands which I have seen, I am compelled to believe that he has not fully comprehended the true pathology of this affection. He has, however, clearly shown the relation of this affection to tuberculosis of the urinary organs, and that alone is worthy of the highest honor.

Dr. Terrillon's article is too long to be given in full, but a few condensed extracts will show his views upon the subject. His description of the symptoms and the general appearance of the parts affected are so much more complete than my own that I prefer to give them in full:

“The fungoid growths show themselves usually at the surface of the urethral orifice. They are projecting and pedunculate. Seldom isolated, they form most frequently a wreath, more or less regular, around the orifice of the meatus. In very aggravated cases they are united into a mass and then form a real projecting tumor with a fringed aspect, of a lively red. In the centre of the tumor is easily to be found the orifice of the urethra masked by those papillary growths. The symptoms of fungoid excrescences of the urethra accompanying tuberculosis of that organ and the bladder includes the observation of two distinct parts: First, the study of the growths themselves and the character of them. Second, all the phenomena to be found in cystitis and tubercular urethritis. Sometimes the symptoms of the two lesions are found together; sometimes, on the contrary, they exist singly up to a certain period of the disease. One of the special symptoms of this affection is the exquisite tenderness of which these fungoids are possessed. The least touch, the least rubbing, the passage of urine suffices to cause the most extensive pain, which renders life insupportable. This hyperesthesia, which may extend to the neighboring parts, causes at the sides of the orifice of the vulva symptoms of the most acute vaginitis. These are the ordinary symptoms of fungoid growths when existing externally.” The author at this point refers to excrescences found within the urethra as being of the same nature as those found at the meatus. He makes no distinction between the two forms of disease. There is, however, a difference worthy of notice. Excrescences found within the urethra are usually cystic polypi or enlarged papillæ of the mucous membrane, conditions which may exist independently of tuberculosis. I infer from some other statements made in his writings that the granular urethritis—as we are in the habit of call-

ing it—is generally secondary to the disease of the urethral glands. The views of this author in regard to the order of development of urethritis, cystitis, and finally tuberculosis of the lungs, are set forth in the following:

“Sometimes, at the time of their appearance, these fungoids appear to be altogether isolated from all other serious lesions. Yet they seem to precede tuberculization, or soon take a rapid course in developing granulations in the urethra. In other cases these growths may appear some time after the symptoms of tuberculization have been established.” The cases recorded by Dr. Terrillon, and also those which have come under my own observation, show that, as a rule, this disease of the urethra precedes the appearance of tuberculosis in other organs of the body, such as the lungs. It also is one of the first lesions observed in tuberculosis of the urinary organs. The following is from Dr. Terrillon’s paper on this part of the subject:

“Now comes up the important question, whether these polypi of the mucous membrane should be considered as a primary or an idiopathic lesion, and I think that it can be solved in the following manner: These polypi are most assuredly the result of chronic inflammation and an irritation of the mucous membrane. Now, development of tubercular granulations within the mucous membrane is at first the cause of irritation, before any changes in the urine; ulceration does not occur until after a sufficient length of time. With one of our patients the first irritation induced the formation of polypi, and the common painful symptoms followed. Their extirpation gave relief, but that lasted only up to the time when urethro-vesical ulceration occurred. It will be observed that in this case the affection began in the urethra and extended to the bladder, and also secondarily involved the left kidney (ascending tuberculosis), causing finally change in the urine, with the free formation of pus. I therefore do not hesitate to maintain that the fungoid polypi are the result of tubercular irritation of the mucous membrane of the urethra, which gives rise to the very serious symptoms which occur in the early stages of the disease. Without them urinary tuberculosis would not give rise to those striking symptoms until after a sufficient length of time, when the ulcerations appear in other organs. An analogous phenomenon, which is observed in the larynx, should be mentioned here. We know, as a matter of fact, that the tuberculization of the larynx does not only occasion ulceration, but also polypoid growths. There is produced at the expense of the ulcerated mucous membrane an hypertrophy and proliferation, in the form of cauliflower excrescences or cock’s comb growths, a species of polypi smaller or larger, by which the glottis might be more or less obliterated. It will therefore be admitted that there is a resemblance between laryngeal excrescences and those found in the urethra of women.

The polypoid excrescences of the female urethra show, from an etiological point of view, to be of two distinct varieties. The first variety is idiopathic, and may be recognized by a slight irritation. The prognosis is good; extirpation in these cases gives a rapid cure. This is the most frequent variety. The second kind, although they give the same outward appearances as the first variety, are on the contrary accompanied from the outset by urethritis and tubercular cystitis, of which variety these lesions constitute important symptoms.

It is clearly evident to me that the two varieties described by Dr. Terrillon differ very essentially in their pathology. The first or simpler forms correspond to the papilloma occasionally seen, and so easily cured by extirpation. The other variety have their origin in tubercular disease of the urethral glands, and are incurable by any treatment heretofore known, as the author states.

Dr. Terrillon gives the full history of four cases observed by him. They are original and of great value, but too long to be produced here. Suffice it to say that in all four there were present the excrescences at the meatus urinarius, due, as their clinical history show, to disease of the glands, and finally tuberculosis of the urethra, bladder, and lungs. A careful post-mortem examination was made in the fourth case observed, which revealed tuberculosis of the urethra, bladder, right kidney, and lungs.

The first case of this kind which I remember having seen came under the care of Prof. E. N. Chapman at the Long Island College Hospital, while I was his assistant. She presented at her first visit the history and physical signs of what was then supposed to be caruncle, which was treated with caustics. Very little relief followed. She soon gave evidence of cystitis, which was also treated for several months without success. The diagnosis made was inflammation of the bladder. After a time she disappeared, but I subsequently learned that she died in the City Hospital, of pulmonary tuberculosis. Upon reflection I am satisfied that her primary disease was tuberculosis of the urethral glands.

The next case came under my own care in the college hospital. When first seen she had papillomatous excrescences at the meatus and cystitis, presumed to be non-specific. I was at that time unaware of the presence of the urethral glands, and therefore did not at first suspect tuberculosis. Treatment gave her no relief, and her sufferings were beyond description. In the hope of curing her, I made an artificial vesico-vaginal fistula, which relieved her very much, but her general condition became more and more like that of a consumptive. She died, and a post-mortem examination revealed complete destruction of her left kidney from tuberculosis. The bladder and urethra were covered throughout with

tubercular ulcerations. Since I discovered the urethral glands I have seen two cases of tuberculosis of them. One of them is now under my care, and her history, so far as it is developed, is as follows:

A young single lady, who first consulted me for dysmenorrhea and frequent and painful urination. I found by examination that she had an antelexion of the uterus, and inflammation of the urethral glands. The painful menstruation was partially relieved by correcting the flexion. The inflamed glands were treated in the manner to be hereafter described, and the inflammation at that point disappeared. Her frequent urination did not subside, however; on the contrary she developed a marked cystitis, which does not yield to treatment. Her lungs at the same time gave evidence of tuberculosis, and now she is nearing the end of her days.

When I found inflammation of these glands associated with tuberculosis of other organs, it occurred to me that the disease of the glands might be of the same nature, or tubercular, but I am indebted to the writings of Dr. Terrillon for the full knowledge of the pathological relations of the affection of these glands to tuberculosis of the other urinary organs. We have studied the subject from different standpoints, and the combined results of our labors cover the ground pretty thoroughly. While he has clearly settled the relation of these excrescences to tuberculosis of the urinary organs, I have satisfied myself that these new growths are but the products of a tubercular inflammation of the urethral glands, the existence of which were, I presume, unknown to him. The treatment of the various forms of inflammation of those glands may all be discussed at the same time.

It is settled upon the best evidence that when these glands become inflamed there is no natural tendency to their recovery. Those who have read the history of my first published case will remember that I employed all the recognized treatment for caruncle, but at the end of a year my patient was no better. Dr. Terrillon has had a similar experience. On this point he says: "A characteristic more important, and to which I desire to call especial attention, because it indicates well in my opinion the consecutive development of these excrescences, is their tenacity and the facility with which they recur. Really, one can see in the observations (meaning his cases) in which continued surgical intervention has been practiced, it brought about either no relief or only a momentary amelioration."

This experience in the treatment of these excrescences is proof positive to me that they originated in inflammation of the urethral gland. My own observations have given the same results, showing clearly that these growths cannot be arrested until the inflammation of the glands which

causes them is eradicated. It is otherwise with polypoid excrescences due to other causes. If they are completely removed they do not return as a rule.

The treatment which I employed at first was to inject the tubules with the ordinary solutions used in the treatment of inflammation of mucous membranes, using for the purpose a hypodermic syringe, with the point of the middle rounded off. This method I found useful, but very tedious. It then occurred to me that laying open the tubules their whole length and keeping them open would prevent the purulent accumulation (which acts so effectually in keeping up the inflammation) and also bring the affected parts within easy reach of the necessary treatment. This method was suggested in my paper published last winter, and since then I have tried the method in quite a number of cases and found it entirely satisfactory. In the majority of cases it is all that is required to effect a complete cure. The method of operating is as follows : The patient is placed upon the left side, and a Sims speculum used to keep the labia apart and retract the perineum. This brings the parts well into view, and within easy reach of the operator.

The position and depth of the tubules having been first ascertained, the probe-pointed blade of a very fine scissors is then introduced, and the posterior wall divided its whole length. To prevent the parts from re-uniting, a small piece of cotton, saturated with persulphate of iron, should be packed in between the divided edges. Brushing the surfaces over with the iron, without using the cotton, will answer, although less certainly, to prevent re-uniting. Very little after-treatment is required. In the majority of cases recovery follows the operation of laying open the canals. Sometimes the inflammation lingers in a modified form, but yields to a few applications of nitrate of silver or sulphate of zinc. In several cases in which the excrescences were abundant, they remained after the operation, although very much reduced in size. An application of nitric acid destroyed them, and they have not shown the least disposition to return.

A CASE OF CHRONIC EMPYEMA WITH EXTENSIVE AMYLOID DEGENERATION—RECOVERY.

REMARKS ON THE SUBJECT OF AMYLOID DEGENERATION, BY P. H. KRETZSCHMAR, M.D., AND B. F. WESTBROOK, M.D.*

The case which we are about to present is one which involves questions of the greatest interest and importance.

The close relation which exists between long continued suppuration and amyloid, or lardaceous, degeneration of the liver, kidneys and other viscera, has long ago been demonstrated, and will only be *illustrated* by our case. The formation of what may properly be called "compensatory lateral curvature" in cases of chronic pleuritic disease, is beautifully shown in this instance, and again reminds us of nature's kindness, even in cases of severe suffering. The mode of surgical interference, as applied by us, is by no means new or original, but it has proven to be, under the existing circumstances, of the greatest value. The most remarkable portion of the report of our case is its termination. Recovery has been occasionally observed in well-authenticated cases of amyloid degeneration, when the disease had not made much progress, but almost all modern writers declare the prognosis to be unfavorable. We could find but one case reported* like ours, where recovery took place after the liver had undergone such changes as to become about twice its normal size, and after the kidneys had shown undisputable signs of well-marked amyloid degeneration.

The patient came under Dr. Kretzschmar's observation during the early part of January, 1879, with the following previous history, as kindly given by Dr. Henry Loewenstein and the parents:

Emily G., born 1863, in this city, of German parents; family history good; had measles during childhood, and enjoyed good health until May, 1877. At that time she contracted pleuro-pneumonia on the left side. After she passed through a very severe attack of that disease, signs of chronic suppurative pleurisy became apparent, and the life of the young girl was endangered. Dr. Loewenstein, under whose care the patient was at that time, aspirated the left side on Sept. 20, 1877, and removed at that time sixty-five ounces of pus. The patient felt much relieved after the operation, but a large amount of fluid accumulated rapidly, and a second time it became necessary to resort to surgical in-

* Frerich's Diseases of the Liver. Translated by Dr. Charles Murchison, New Syd. Soc., 1861. Vol. II., p. 185.

terference. On September 28th, seventy-two ounces of pus were drawn off. Soon afterwards the pleural cavity became again filled with pus, and, as is sometimes observed, ulceration of the serosa took place; the pleura costalis became partially destroyed; the pus found its way through the soft parts of the chest, and a tumor of considerable size formed in the left mammary region, beneath the skin. This finally broke, and a large amount of pus was discharged externally. This occurred during the latter part of the month of October, 1877. Within a week or two after the pus had commenced to flow, a number of other openings formed and kept on healing and breaking again. The patient soon became exceedingly weak; she lost a great deal of flesh, and, according to the testimony of her mother, she rapidly passed into a "decline." During the month of November a peculiar enlargement of the abdomen was first observed.

When Dr. Kretzschmar took charge of the case in January, 1879, he found the patient in a condition which may be thus described:

She is remarkably anæmic, very emaciated, and has a cachectic appearance; her face has an old and worn expression. The extremities are cold, and the flesh over the entire body is soft and flabby. Over the left mammary region there is a soft tumor, with three openings leading into it; a considerable amount of bad pus running from these channels, increasing on pressure. On examination it is found that these openings communicate with each other, but it is impossible to detect any place where the wall of the chest is perforated. There is not much pain or tenderness, except on probing. A marked deformity of the thorax is observed; the left side is very much contracted, and the right side abnormally expanded. There is also a very well-defined lateral curvature of the spine in the lower part of the cervical and in the dorsal region, with its concavity towards the left side, and a compensatory curve in the lumbar region. A large swelling in the region of the liver attracts attention. This tumor extends downward beyond the umbilicus and upward beneath the ribs; to the touch it is rather hard, but smooth, and closely resembling an enlarged liver. The spleen and the lymphatic glands do not show any abnormalities. The circumference of the abdomen, taken at the terminus of the true ribs, is 84 C. (about 34 inches); at the umbilicus it is 90.5 C. (about 36 inches). On percussion of the chest increased resonance is found over the entire right side, extending downward as far as the sixth rib; below there is absolute flatness. At the apex of the left side the sound is fair, but there is nothing like pulmonary quality to be heard below the second rib. Auscultation reveals compensatory exaggeration of the respiratory murmur over the right side; below the sixth rib none can be heard. On the left side the respiratory murmur is

diminished at the apex and absent below the second rib. On measurement it is found that the right side is 5.5 C. (about 2 inches) larger than the left. The apex beat of the heart is located in about the median line.

A provisory diagnosis of pleural fistula and waxy or amyloid liver was made after the first examination. Ordered to continue tablespoonful doses of cod liver oil, to take twenty-five drops of dialyzed iron three times daily, and to apply iodide potassium ointment to the hepatic region.

Under this treatment the patient's general health seemed to improve somewhat, her appetite became better, and she gained strength enough to go out and take short walks. However, the discharge of pus continued, the amyloid liver still grew larger, and on inquiry it was found that the patient had to get up several times during the night to urinate, and that she passed more water than formerly. Measuring the amount passed during twenty-four hours for a number of days during the month of March, the following table was obtained:

March	5	2,100 C. C., about 4½ pts.
"	6	2,500 C. C., about 5½ pts.
"	7	2,800 C. C., about 5¾ pts.
"	19	2,400 C. C., about 5¼ pts.
"	20	2,300 C. C., about 5 pts.
"	21	2,700 C. C., about 5½ pts.

The examinations of the urine made at that time gave, with slight variations, the same result, viz.:

Very low specific gravity, ranging between 1,004 and 1,008; exceedingly pale appearance, almost resembling water; presence of albumen in a high degree, amounting to a sixth or an eighth of the volume; presence of a few hyaline casts and of considerable amount of epithelium.

The amount of urea, chlorides and phosphates excreted was not investigated.

The history of the case, the presence of the large tumor in the region of the liver and its peculiarities, together with the symptoms pointing towards an affection of the kidneys, led Dr. K. to make the positive diagnosis of amyloid degeneration of liver and kidneys.

Œdema of the lower extremities developed about the end of April, and corroborated the diagnosis.

During all this time the patient had been kept on the sustaining treatment as given above, without obtaining very favorable results. Sulphide of calcium in 0.01 ($\frac{1}{10}$ grain) doses was then added to the remedies in use, and given three times daily for over two months. An injection of carbolated water into the fistulous openings twice or three times a day was also ordered and employed from April till the beginning of October.

In the latter part of May the patient consented to present herself at Prof. Armor's clinic, at the Long Island College Hospital. At that time

the circumference of the abdomen taken over the ninth rib, had increased to ninety C. (about thirty-six inches), and the patient, when walking, presented the picture of a pregnant woman. Prof. Armor related to the class the history of the case. He went carefully over all the signs and symptoms present, and arrived finally, by the mode of exclusion, at the same diagnosis as that given above. As to the prognosis of the case, his remarks were short and unmistakable. He said, "the doctor"—meaning Dr. K.—"will do all he can to sustain her, but amyloid degeneration is regarded as an incurable disease, and sooner or later the patient will die from asthenia."

Several medical gentlemen saw the case without suggesting anything new in the way of treatment, until at a meeting between the authors of this paper, it was agreed to try surgical interference, as a kind of forlorn hope.

October 7th the operation was performed by Dr. K., Dr. Westbrook assisting. After the administration of an anæsthetic, half ether and half chloroform, the several outlets were laid open, and it was found that the small aperture leading into the pleural cavity was located between the second and third rib. The muscular tissue between the two ribs was removed, and the opening was made large enough to allow the introduction of the index finger. The patient was placed in such a position as to facilitate the discharge of the large amount of decomposed, stinking pus which was contained within, and the cavity was afterwards well washed out with a strong solution of sulphate of zinc and carbolic acid.

A fountain syringe of a rather primitive nature was made by fastening to a plain tin funnel a piece of rubber-tubing about half a meter (19 inch) in length, to which in turn one of Jacques' soft rubber catheters was attached. The catheter, after being well lubricated with carbolic oil, was introduced its *entire* length into the pleural cavity, and the funnel being elevated as much as the rubber tubing allowed, the cavity was washed out thoroughly three times a day, with as much as a half liter (about one pint) of a 2% carbolic acid solution. Within a week after the operation a 1% sulphate of zinc solution was prepared, and a half liter was used once daily as a wash in the same manner as just described. No change in regard to medication; cod liver oil, dialyzed iron, iodide potassium ointment and the best of food were relied upon. The amount of pus which was discharged immediately after the operation and for a long time after the use of the fountain syringe, was something marvelous, and its odor was at first simply disgusting.

The patient passed through the operation without the slightest difficulty, and from that day she steadily and continuously improved until now she has recovered such a degree of health as is possible under the circumstances.

In December, 1879, Dr. K.'s attention was called to the fact that the girl's dresses were getting to be too wide around the waist: the tumor had commenced to diminish in size. At the same time the discharge from the pleural cavity had almost ceased to flow, and it was with difficulty that the soft rubber catheter could be introduced. A week or two afterwards, early in January of this year, the opening leading through the walls of the chest had become so small that the use of the fountain syringe had to be discontinued, and a hard rubber syringe with a very fine nozzle was employed. By the end of January the discharge of pus ceased, and a few days later the opening had closed up. A large scar indicates at present where the walls of the chest had been perforated; the left mammary gland has been destroyed by the operation.

The amyloid liver continued to decrease in size, the circumference of the abdomen became about one centimeter ($\frac{4}{10}$ inch) less every month. The amount of urine excreted during 24 hours came down to an average of 1,800 C.C. ($3\frac{3}{4}$ pts.) during February, and to about 1,500 C.C. (3 pts.) during March. Repeated examinations made during that time revealed a constant decrease in the amount of albumen excreted, until there was no trace of it found during the latter part of March.

At the same time the mammary gland on the right side developed; the patient gained in flesh, and a moderate amount of adipose tissue was deposited at the usual parts of the body. The patient could undertake long walks, she enjoyed dancing, and began to assist her mother in doing such housework as does not require an especial amount of bodily strength.

In May, 1880, the patient was again presented at the medical clinic in the Long Island College Hospital, by Prof. Armor. After going over the history of the case and referring to the statements made a year previously, the Doctor dwelt at length on the peculiar changes which had taken place in the girl's condition during the last twelve months, and closed his remarks with the expression that he expected to see the patient return in another year, entirely cured.

In June the girl menstruated for the first time, and she has been regularly unwell since, with one exception, in August.

During the latter part of June a physical examination was made and the following notes taken:

Percussion—Right side: Resonant from apex of the lung down to the seventh rib in front, and to the ninth rib in the axillary region. Posteriorly resonant to the tenth rib.

Left side: Resonant from the apex to the third rib, with dullness below, extending one centimeter ($\frac{4}{10}$ inch) to the right of the sternum in front. In the axillary region, flatness to the eleventh rib, about 5 C. (2 inch) above the crest of ilium. Posteriorly, dullness from the middle of the scapula to the eighth rib, with flatness below that point.

Auscultation—Right side: Respiratory murmur corresponds with the percussion; posteriorly, creaking (pleuritic) sounds are heard in the interscapular region and anteriorly in the sub-clavicular region, near the median line.

Left side: At the apex the respiratory murmur is weak with prolonged expiration; below the third rib a very feeble sound can be heard; below the fourth rib there is entire absence of respiratory sounds, as also in the axillary region. Posteriorly the murmur is weak from above down to the sixth rib and absent below.

The *heart* is in about its normal position, displaced somewhat to the right side.

The following *measurements* were made at the same time:

Circumference of the abdomen, taken at the lower end of the ninth rib on the right side, 59.7 C. (about $23\frac{1}{2}$ inch). At the same place on the left side, 58.6 C. (about 23 inch). Over the umbilicus, 71.4 C. (about $28\frac{1}{4}$ inch). The last rib is 2.3 C. (about 1 inch) higher on the right side than on the left, and about $7\frac{1}{2}$ C. (3 inch) above the crest of the ilium.

Circumference of the thorax, taken at the third intercostal cartilage, 61 C. (about 24 inch); the right side measuring 32.3 C. (about $12\frac{3}{4}$ inch); the left side measuring 28.5 C. (about $11\frac{1}{4}$ inch); at the fourth costal cartilage 66 C. (about 26 inch); the right side measuring 37 C. (about $14\frac{1}{2}$ inch); the left side measuring 29 C. (about $11\frac{1}{4}$ inch).

The following comparative table illustrates the changes as they took place in the size of the amyloid liver:

Circumference of abdomen taken at the ninth rib: Jan., 1879, 84 C. (34 inch); May, 1879, 90 C. (36 inch); June, 1880, 59 C. (23 inch).

At the umbilicus: Jan., 1879, 90.5 C. (36 inch); June, 1880, 71.4 C. ($28\frac{1}{4}$ inch).

It may finally be stated, that although the patient has not been under any physician's care for the last three months, she still takes her cod-liver oil and dialyzed iron; since the beginning of her sickness she has taken over 7 liters (two gallons) of the former, and a liter and a half (three pts.) of the latter.

P. H. K.

REMARKS.

The post-mortem appearances of amyloid degeneration were noticed by many of the older authors, but it was considered a variety of fatty degeneration till Rokitansky gave a more exact description of its appearance in liver and spleen, and bestowed upon it the title of lardaceous (*speckig*) infiltration.* He associated it with diseases of the vegetative system, "especially with scrofulous and rickety disease, with syphilitic and mercurial cachexia." He also noted its development as a sequel to intermittent fever. In accordance with the humoral pathology of the day, he considered it an infiltration or deposit from the blood.

Virchow soon found that this lardaceous matter gave a peculiar reaction, which led him to class it with the carbo-hydrates, and to name it *amyloid matter* (animal cellulose). By means of this chemical test Meckel also identified it in the kidney and intestines; and Virchow ascertained its presence in the lymphatic glands and other structures.†

More exact chemical analyses, made subsequently, by Friedrich, Kekulé, and others, showed that it belonged among the nitrogenous substances, though its exact relation to albumen has not been ascertained. Dr. Dickinson has supposed it to be a dealkalized fibrin, resulting from the great loss of alkaline salts in cases of copious and long-continued suppuration. His views have not been confirmed.

An enumeration of the localities in which the amyloid deposit has been found would include almost all the organs and tissues of the body. Its favorite localities are the viscera of the abdomen, viz.: the liver, spleen, kidneys, and the mucous membrane of the intestinal canal.

It is also of frequent occurrence, relatively, in the lymphatic glands.

More rarely its presence has been noted in the mucous membrane of the urinary and of the respiratory apparatus; in the ovary and testicle; in the thymus and thyroid bodies, and supra-renal capsules; in the heart and the intima of the great blood-vessels, in the subcutaneous connective tissue, and that of the mesentery.‡

It is found, usually, in two or more of these localities. As a purely local deposit it has been seen by Billroth in single lymphatic glands, and

* Carl Rokitansky, *Manual of Patholog. Anat.* Translated by E. Sieveking, under the auspices of the Sydenham Society. Vol. II., p. 100. Phila.: Blanchard & Lea, 1855.

† *La Pathologie Cellulaire.* 4th French ed., translated from the 3d German ed., by Dr. Is. Strauss. Paris: 1874; p. 424.

‡ Hayem, see Charcot, *Maladies du Foie et des Reins.* Paris: 1877; p. 342.

in the outer layers of old blood extravasations. Birch-Hirschfeld* found it in the mesenteric glands of a person who had suffered from enteric fever, and it has been observed by others in inflammatory new formations, near old gummy deposits, in the ocular conjunctiva, etc.†

The amyloid substance is usually found in the walls of the arterioles and capillaries. It also invades the epithelial cells, and the cells of connective tissue, and parenchymatous organs.

Thus, in the kidney, the earliest change is found in the vessels of the glomeruli and the small arteries elsewhere; but it gradually invades the tubuli, and their epithelial lining.

In the lymphatic glands, the vascular walls present the appearances peculiar to this condition, and the lymph corpuscles are found to have undergone the same change.

In the case of the liver, the organ in which we are particularly interested, the opinions of good observers differ as to the primary seat of the deposit.

It is claimed by Meckel, Rindfleisch,‡ and others, that the change commences in the hepatic cells; but others are equally positive that the vascular walls, here, as elsewhere, are the seat of the initial lesion. We are not in a position to decide the question at present. The vessels affected are the branches of the hepatic artery.

If the deposit is extensive, the organ is enlarged, pale, anæmic, and its cut surface has a peculiar waxy, translucent appearance, resembling the cut surface of bacon. Hence the name lardaceous (*speckig*) applied to it by the Vienna school. When it is more limited in extent, certain more or less circumscribed areas presenting the waxy appearance may be observed. In the kidney, the glomeruli are rendered visible as small whitish specks in the cortical substance. But the change may be so slight as to elude detection by the naked eye.

If an organ presenting the appearances detailed above be treated with a watery solution of iodine, or of iodine and iodide of potassium, it assumes a dark, reddish brown tint comparable to the color of mahogany. This is easily distinguished from the light yellow color which it imparts to healthy tissues. The change of color may be uniform over a considerable extent

* Real-Encyclopædie der Gesamnten. Heilkunde Herausg. Von Dr. Albert Eulenburg. Wein: 1880, s. 277.

† Birch-Hirschfeld, *loc. cit.*

‡ Doctor Evans has called our attention to the fact that R. does not say exactly what we here attribute to him. He lays more stress upon the hepatic cells than do other authors, but does not assert that the process begins in them. Having described fatty infiltration in the preceding section, he says: "Were we to content ourselves with a rough sketch of the histological details, we might simply substitute 'lardaceous' for 'fatty' matter." Vol. ii., p. 85.

of surface, or, it may, *e. g.*, in the kidney, be in lines or dots; the lines, in this instance, representing the small arteries, and the dots the glomeruli. In the liver, according to our observation, the change is usually distributed very irregularly; but, in some instances, large areas are seen to be almost completely converted into this new material. A remarkable illustration of this extensive degeneration was furnished by a case of pyo-nephrosis of calculous origin under the charge of Dr. Frank W. Rockwell. At the autopsy, at which we assisted, the liver was found to have attained at least twice its normal size, and seemed to be almost entirely converted into amyloid matter. On the addition of sulphuric acid to the parts that have been already treated with iodine, the brown color slowly changes to a blue or violet, varying somewhat in different cases. The same effect is produced by adding a solution of zinc chloride or calcium chloride.

A solution of methylanilin (1-100)* has the property of giving to the amyloid matter a bright red color, while the surrounding tissues which are not degenerated take a blue or violet tint.

This reaction is particularly adapted to histological uses.

The enlargement of the organs is due to thickening of the vascular coats and swelling of the cellular elements.

Thickening of the walls of the blood-vessels by narrowing their lumina, also accounts for the anæmic condition of the degenerated parts. But this thickening rarely causes entire occlusion; so that an amyloid liver may be successfully injected.†

Owing to the derangements in the vascularity of the affected organs, as well as to the altered structure of their cells, there are marked derangements of function.

The kidneys separate an increased amount of highly albuminous urine.

The liver fails to contribute the proper quantity of bile for the carrying on of the digestive function, and, according to Frerichs,‡ its glycogenic action is also at a standstill, as it frequently is in jaundice.§ Degenerated lymphatics fail to transfer and elaborate the lymph, and the functions of other organs are in like manner affected.

We shall omit the enumeration of those exhausting diseases which precede the development of the amyloid dyscrasia, and turn our attention to the theories which attempt to account for its production. Rokitsky

* Birch-Hirschfeld, *loc. cit.*

† Rindfleisch, *Pathological Histology*, New Sydenham Soc. Translation.

‡ *Diseases of the Liver*, by F. T. Frerichs. Translated by Charles Murchison, New Syd. Soc., 1861. Vol. II., p. 173.

§ Dr. J. Wickham Legg., *On the Bile, Jaundice and Bilious Diseases*. New York: D. Appleton & Co., 1880; p. 364.

attributed the lesions to the presence in the body of an abnormal material, which circulated through the blood-vessels and was deposited in the tissues.

A similar view is favored by Virchow, Rindfleisch, Frerichs and others, and the humoral idea has been still further developed by Dickinson, whose theory, as given by Murchison,* is as follows: During prolonged suppuration, the loss of alkali in the pus is so great and continuous that there is not enough left for the metabolism of the albuminoid substances, and, in consequence, a substance resembling fibrin, minus a certain amount of alkali, is deposited in the tissues.

This theory is based upon the fact that immersion of an amyloid liver in an alkaline solution deprives it of the property of reacting with iodine and sulphuric acid.

Further, if, according to Dr. D., fibrin be immersed in dilute hydrochloric acid (1-10,000), so as to extract its alkaline constituents, it is converted into a gelatinous mass exhibiting the reactions of amyloid substance.

Charcot, in his lectures on Diseases of the Liver and Kidneys, dwells at some length upon this hypothesis, though he does not adopt it.

The opponents of the humoral doctrine base their objections upon the following facts: First, that the amyloid material has not been found in the blood of subjects afflicted with this dyscrasia; second, that the change is confined to particular organs, sometimes being exhibited by only one or two; third, its occurrence as a purely local phenomenon, *e. g.*, in old blood extravasations, as shown by Friedrich and Billroth, and, physiologically, in the hyaline cartilages of old subjects;† fourth, that in certain cases, when the solution of methylanilin is applied, its characteristic reaction is obtained in some localities, while in others middle tones between red and violet are produced, as if the tissues were only partially degenerated.‡ It is impossible, in the present state of our knowledge, to say which of these theories is the correct one.

In any case, we think it necessary to assume a *predisposition* on the part of the affected organs, otherwise it would be impossible to explain the irregularity of distribution which characterizes this disease. The theory of Dickinson, while attractive by virtue of its simplicity, must, for the same reason, be received with caution. Pathological processes are, as a rule, very complex.

In addition to the objections mentioned above as applying to the humoral theory in general, it may be said, in regard to this particular hypothesis, first, that the amyloid degeneration occurs in some cases of syphilitic dis-

*Chas. Murchison, *Clinical Lectures on Diseases of the Liver*. London: Longmans & Co., 1877. Dr. Dickinson's paper is in the *Med. Chir. Transactions*, Vol. L., p. 55.

† Virchow. *Path. Cell.*, p. 432.

‡ Birch-Hirschfeld, *op. cit.*

ease, where the suppuration is not excessive; and, second, that it has been known to develop after the suppurating sore had healed, and this in cases of simple varicose ulcer.* It may be said, in favor of the humoral theory, that a purely local process, *e. g.*, necrosis of one of the long bones, could not give rise to disease in a distant organ except through the medium of the circulation, and that the simultaneous affection of organs which differ so entirely in their structure and function as do the liver, spleen, kidneys and mucous membranes, would indicate the presence of some humor, conveyed to them by the blood. The fact of recovery taking place occasionally seems to favor the humoral theory.

The clinical history of amyloid degeneration is sufficiently illustrated by the above case. Among the complex of symptoms, it is difficult to say which are due to the primary cachectic condition and which to the local changes. We can say, however, that the albuminuria is due to degeneration of the renal glomeruli; also that the deficient formation of bile, as shown by flatulence and light-colored stools, is the result of the destruction of the hepatic cells.

The emaciation, in addition to that caused by the albuminuria, when it exists, and by the primary disease, may be accounted for by the abolition, partial or complete, of the glycogenic function of the liver. The anasarca may, as Virchow has pointed out, be partially a consequence of the lymphatic disease. The diarrhea and disturbances of digestion are due either to the pressure of the enlarged liver or to degeneration of the muscular and mucous coats of the intestine.

The treatment of cases of amyloid degeneration must be twofold. As a preliminary measure, it is necessary to cure the primary disease. Unless this can be done the prognosis is absolutely bad. Where it is possible to remove the cause, it must be done as speedily as possible, for two reasons: first, because, if allowed to continue for too long a time, the degeneration may reach a point where it is incurable; second, because it is dangerous to perform any serious operation upon one who is a subject of advanced amyloid disease.† A majority of these cases, which have been reported as cures, have been of syphilitic origin, and the success, such as it has been, is to be attributed to the influence of the specific treatment upon that disease. The importance of early operations upon cases of necrosis is evident. Murchison justly says that the surgeon who is too anxious to save a limb may sometimes sacrifice a life, by deferring his operation until incurable amyloid disease has developed.

If the primary affection is amenable to treatment, the second indication, *viz.*: the removal of the lardaceous matter, may be attended to. There is no specific treatment for this, unless that advocated by Dickin-

*Birch-Hirschfeld, *op. cit.*

†Sir James Paget, *Clinical Lectures and Essays*. D. Appleton & Co., N.Y., 1875, p. 403.

son should prove to be successful. In accordance with his theory, he recommends the use of alkaline salts of the vegetable acids, and carbonates, not only after the disease has developed, but as a prophylactic in cases of chronic suppuration.

Dr. Budd recommended the use of the muriate of ammonia to reduce hepatic enlargements, and dilute nitric acid for the kidneys. Cod liver oil, particularly for children, iron, quinia, and the iodine compounds have also been used. But perhaps the best guide will be the accounts of the cases in which recovery has been supposed to occur. The one which admits of least doubt is that referred to in the first portion of this paper.

We give an outline of it as we find it reported by Frerichs in his work upon Diseases of the Liver. The patient was an adult male, who suffered repeated syphilitic infection. There was necrosis of one femur, in addition to other signs of secondary syphilis. He had taken several courses of mercurial treatment; albuminuria; anasarca. The liver measured 18 C. M. (7 in.) in the mammillary line. The spleen extended 7 C. M. ($2\frac{3}{4}$ in.) below the costal cartilages. He was first treated with the iodide of iron, and subsequently with the mineral waters of Pyrmont,* and lactate of iron. Under this treatment he improved greatly, and went to the south of France for the winter.

While there, in consequence of chills and an improper diet, together with unsuitable treatment in the shape of blisters and purges, the disease became aggravated.

On his return he was given warm baths, with the iodide of iron, and an "unirritating diet." After many months other iron preparations were administered. The anasarca disappeared; the albumen in the urine diminished to a mere trace. After seven months the liver dullness extended but 10 C. M. (4 in.), and the spleen was 4 C. M. ($1\frac{1}{2}$ in.) above the border of the cartilages.

Another case, reported by Frerichs† as a cure, was that of a naval officer, who had repeatedly taken mercurial courses for the cure of various secondary syphilitic affections. He had pseudo-rheumatic pains and an obstinate gastro-enteric catarrh, with jaundice. There had been ulceration of the pharynx. The liver measured 16 C. M. in the mammary, and 10 C. M. ($6\frac{1}{3}$ and 4 in.) in the sternal line, and was painful. The spleen was also enlarged.

* The waters of Pyrmont: Carbonates of iron, manganese, calcium, and magnesium; chlorides, sulphates, nitrates, etc., with a considerable amount of free carbonic acid. The temperature is about 12° C.—Dr. L. Lehmann, *Bäder u. Brunnenlehre*. Bonn. 1877; p. 315.

† *Opus Cit.*, p. 186.

He was sent to Aix-la-Chapelle, where he was first cured of his gastro-enteric catarrh by chalk and opium, then directed to take the baths, and afterwards to use the thermal waters,* with the iodide of potassium. After four weeks the "jaundice disappeared, the liver returned to its normal dimensions, whilst the pseudo-rheumatic pains were almost completely removed." The diagnosis in this case is, to say the least, doubtful.

Dr. Murchison† has recorded the case of a man, æt. 28, who had been in the army in India, and while there had syphilis with secondary symptoms, but no bone disease. Two years before admission to the hospital he was taken sick with violent pain in the hepatic region, etc. He was sent home, and recovered sufficiently to work as a laborer for about a year. He was then admitted into the Fever Hospital with the following symptoms: Gastro-enteric catarrh; copious secretion of urine (sp. gr. 1,010–1,015), always containing much albumen, and once a few small hyaline casts; no dropsy. The liver measured in the mammillary line $9\frac{3}{4}$ in.; the spleen, vertically, $5\frac{1}{2}$ in.; horizontally, $6\frac{1}{2}$ in., but did not project below the ribs.

He was treated, at first, with mineral acids and bitter tonics, under which treatment he gained 16 pounds. He then had fifteen-minim doses of compound tincture of iodine. After forty days of this treatment the liver began to diminish in size very rapidly, and finally was reduced to $7\frac{1}{4}$ in. in the mammillary line, and the spleen to $4\frac{1}{4}$ in. in the vertical diameter. There was no diarrhea. The urine was of normal quantity, and contained one-twentieth albumen.

Frerichs has seen the enlarged viscera very much reduced in size by the use of the mineral waters of Karlsbad,‡ but the cases ultimately terminated fatally. He deprecates the use of mercury, but we have found a case reported by Mr. Henry Lee§, of a man 38 years old, suffering from secondary syphilis, with pains in the bones; sinuses and discharge of pus from the upper part of the forearm and one rib, which was carious; who was anæmic, very cachectic, and frequently passed water which was

* "The springs of Aix-la-Chapelle are thermal, sulphurous, and saline." Temperature, from 151° to 112° F. The solid matter is mostly chloride of sodium, with a little iodide and bromide of sodium.—Frerichs' Diseases of Liver, Vol. II., p. 183.—Foot note by Dr. Murchison.

† Clinical Lectures on Diseases of the Liver. London: 1877; p. 41.

‡ Temp. 122° – 197° F. They contain free carbonic acid, sulphate of sodium, carbonates, etc., with traces of iron, manganese, iodine, etc. Frerichs, Vol. I., p. 124.—Note by Dr. Murchison.

§ *Lancet*, July 20th, 1868.

"intensely albuminous." He was given a calomel vapor bath every night for eight or nine weeks with nitro-muriatic acid internally, and some slight alterative treatment. He recovered completely. In this case nothing is said about the condition of the liver and spleen, nor was any diagnosis of amyloid degeneration made; yet the clinical history is that of amyloid disease. Other cases have been reported, but they are mostly open to doubt on the question of diagnosis.

Probably a fair statement of the views of the majority of writers upon this subject would be that they admit the *possibility* of a cure, provided the degeneration has not gone too far and the cause can be removed. *As far as we know, no case has yet been reported where the disease was so far advanced, the diagnosis so sure, and the cure so complete as in ours.*

B. F. W.

BRIEF REVIEW OF THE SOCIETY'S LATE DISCUSSION ON VACCINATION, WITH REMARKS UPON THE VIRUS NOW IN USE.

BY DR. J. H. H. BURGE.

The discussion referred to was upon a report of the Committee on Hygiene, entitled "An Investigation into the Causes of Alleged Futile and Bad Results following Vaccinations." See PROCEEDINGS, Oct., 1880, p. 268.

Although I heartily appreciate this well-timed work of the Committee, and especially the resolution which was recommended by them and adopted by the Society, referring the whole subject to the National Board of Health, I desire to call attention to the committee's conclusions as printed on page 271.

I. Drs. Martin, Foster, Bogert and Griffiths are represented as believing that humanized virus, of the first three or four removes, if properly selected, is as effective as true animal virus; and *true animal virus* is defined to be virus descended from calf to calf. Now, I think the history of vaccination will abundantly show that this conclusion is doubly false. If the comparison here made is between humanized virus, originally obtained from natural vaccinia, on the one hand, and virus descended from calf to calf on the other, I believe the latter is deteriorated already, and the former is better, not only to the third or fourth remove, but to the thousandth.

The Committee's second conclusion is, that "humanized virus deteriorates with each remove, while the 'true animal virus' rarely loses its strength." Again, I hold that if the humanized virus referred to is originally from *natural*, and not inoculated, vaccinia, it does not deteriorate, and that the true animal virus, so called, that is, the calf to calf virus, is a poor substitute.

The third conclusion is, "that erysipelas is a disease whose occasional occurrence is inseparable from vaccination with humanized virus," upon which I would remark that erysipelas is a disease whose occasional occurrence is inseparable from vaccination or inoculation with any kind of virus; it is also inseparable from abrasions of the skin from any cause whatever. In this connection Dr. Martin is quoted as saying: "I have seldom known it (erysipelas) to follow any other than a fine arm." On general principles, I have no hesitation in saying that it will follow any kind of an arm, and you will recollect that members of this Society have lately reported cases occurring after the use of bovine virus. The records of the N. Y. Dispensary and Kine-pox Institution are also appealed to in the report, as showing an average of one case of erysipelas in every five hundred vaccinations with humanized virus, whereas no case had ever come to Dr. Martin's knowledge out of thousands of persons known to him to be vaccinated with true animal virus.

Now, it does not seem to me at all surprising that one case of erysipelas should have happened in five hundred vaccinations in the N. Y. Dispensary—indeed, one-fifth of one per centum seems a small proportion, when we consider the character of the children who apply to such an institution. It may also be safely assumed, that the atmosphere and general conditions favorable to the development of erysipelas, are present both in a dispensary where fifty thousand patients a year are treated, and in the homes from which these patients come. I have vaccinated with humanized virus at least two thousand children, and have never in my own practice seen a case of erysipelas follow the operation. My own conclusion, therefore, is, that if the source from whence the virus is taken, and the time and manner of taking it, and the method of its introduction, are all right, and erysipelas follow, we may be absolutely certain that the evil arises from the condition of the child, or his surroundings, and not from the virus or the operation, except, indeed, as an exciting cause.

This position is so important that I should not assume it without the strongest conviction of its truth, and I ask for it the most careful consideration of the Society, that it may be either confirmed or controverted.

The fourth conclusion of the Committee is, to a certain extent, in corroboration of the views which I have just uttered. It is expressed in these words:

“When erysipelas or bad results do occur, they are found in sickly children, or in the opposite class of strong, full-blooded children.”

The Committee's remarks upon the sources whence come the supplies of virus for all classes of the community are very significant. I quote from page 274 of October PROCEEDINGS :

“There are some 13 men, physicians and so-called ‘purveyors,’ who advertise to sell virus. There are many more who profess to furnish it, but who, when called upon to furnish a quantity, evade direct answers and ward off the needed supply to a ‘more convenient season.’ Drug stores sell virus, some from sources that cannot be ascertained. The pure ‘Beaugency stock,’ imported by Dr. Martin, has been drawn upon by new propagators for their first supply. Whether this stock in all cases has been kept up, or the vaccine has been mixed with humanized virus, or has been deteriorated by admixture with pus, dirt, dung, etc., etc., or has been supplanted by humanized vaccine of an inert or dangerous character, your Committee could not ascertain, as it did not have the power to go ‘behind the scenes.’ This duty must be delegated to another medical body, the National Board of Health, which alone has the power to enforce regulations and protect the public from inert or unsafe vaccine virus. Your Committee have reason to believe that such virus is sold, even to the army.”

The Committee conclude their report on page 276, as follows:

“Gentlemen of the Society, if the assumption by your Committee be correct—that unreliable propagators of virus care more for the ‘almighty dollar’ than they do for the purity of their vaccine; that, owing to the lack of any central body to arrange for and maintain pure and unadulterated virus, as is done in England by the National Vaccine Institution, there are unreliable dealers; that many of us have at least been led to believe that the ‘want of susceptibility’ is the main reason why an uncomfortably large proportion of our vaccinations have failed, whereas Dr. Martin tells us that such a plea can be *rarely* conscientiously advanced; if dispensaries, not having an appropriation sufficient to purchase animal virus of pure quality, rely largely upon gifts from various individuals—careless as well as careful ones; if your Committee believes all this, as it certainly does, and further, that with proper management, the purity and reliability of vaccine virus can be assured in the future, * * * * * it becomes a duty for your Committee to do what it can, through you, towards the furtherance of such an end.”

Then follows the resolution which was recommended by the Committee, and adopted by the Society, petitioning the National Board of Health to regulate and control the sale of vaccine virus.

The Committee has done valuable work—I only desire to supplement it. The Committee report various opinions on the use of vaccine crusts. It does seem to me that the use of the *scab* (I like this name best, because it is vulgar) would have been discarded long ago but for the commercial interest which it has; and for the fact that virus mixed up in this way with the inspissated products of common inflammation, can be kept for a long time, and when large quantities are needed for immediate use, as in case of threatened epidemics, a supply can much more conveniently and cheaply be obtained in this form. But will this justify our using an in-

ferior and somewhat dangerous article, except in cases of dire necessity? I think not.

Notwithstanding all that has been said, and all that I intend to say, I believe that the cultivators and purveyors of vaccine virus have, as a rule, been careful, painstaking men, to whom the thanks of the profession are due for their earnest endeavor to keep us supplied with a reliable article. That they have not done so, is due, in my opinion, to a much deeper cause than the venality of a few exceptional traders in virus. Of this, more anon.

My references to the discussion on the Committee's report shall be impersonal, because some of the members who spoke may not be present to-night. The first extols humanized virus; tells how reliable he found it, when, for fifteen years on the Pacific coast, he used no other; but he is favorably disposed towards bovine virus, because parents think it safer, and do not blame the physician if bad results follow. I do not think any amount of non-professional prejudice should influence us.

Another member said he had had a fatal case of erysipelas in a child whom he vaccinated with humanized virus, and he had not used humanized virus since. I would like to ask him whether, in case he should meet with a like misfortune while using bovine virus, he will join the Anti-Vaccination Society? He knows as well as any of us, that no person ever sustained a scratch of the skin without being liable to an attack of erysipelas, and to assume that in this case it was communicated by the vaccine virus is, to my mind, unreasonable. The doctor did not say anything of the source of the virus which he used on that occasion; but we know him well enough to be sure that he took it from a typical vesicle on a healthy child upon the seventh or eighth day after vaccination—that the lymph was transparent, and free from blood or pus, and that the doctor used a clean instrument in transferring it to the new patient. Now I say, assuming all this—as we not only have a right, but are bound to do—I think the doctor is rash in his conclusion. Beside, if I am not misinformed, it is notorious that excessive inflammation and erysipelas are more frequent attendants upon the use of bovine than of humanized virus. I shall be surprised if this statement is challenged. Dr. Read's report strongly corroborates this view.

The next speaker was surprised that any should prefer virus one or two removes from the cow. Let me surprise him still more by remarking that, if I could obtain it, I should prefer the virus which was in general use in this country in 1840, and which Dr. Cook, of Rensselaer County, says was the same which Dr. Moorhouse brought from England in the year 1800.* This virus had passed through the systems of an innumerable

* See Trans. Med. Soc. of the State of N. Y., 1841.

multitude, and the most acute observers could not perceive the slightest change in the character of the vesicle which it produced, nor in the amount of protection which it afforded.

The member of this Society to whom I have just referred, then draws, from the experience of an army surgeon, a case to show the danger of depending upon humanized virus.

A soldier comes home, gets vaccinated, and, about the same time, contracts syphilis from another source. He then returns to the army, his pustule is tapped, and some of its contents pricked into the arms of his comrades. It probably was—as stated—a “pustule” and not a vesicle of transparent lymph. It probably was, also, a case of second or re-vaccination, and, therefore, not fit to be used. It is also a pretty well settled rule that we should not vaccinate from adults under any circumstances. I submit to you, Mr. Chairman and gentlemen, that this case does not prove that there is any danger in using humanized virus.

That there is danger in taking matter from the pustule of a traveling soldier, and pricking it from arm to arm into the circulation of a whole regiment, is very true. Beside, there is danger in the very method employed, and this danger is independent of all questions concerning the purity or impurity of vaccine virus. Take a lancet and go rapidly from man to man, and take off the cuticle from a little spot on each of one thousand arms, and rub no virus in, and what think you would be the result? I would not like to be the tenth man, much less the thousandth, particularly if the men were unwashed soldiers. You may reply, that this experiment has not been tried—that my argument is entirely hypothetical; but I ask you seriously, is not the inference which I would have you draw, based upon abundant human experience? Especially dangerous do I regard this rapid arm to arm process, without the precaution of cleaning the instrument after each use thereof, among crowds of emigrants while they are still on shipboard, after a long voyage.

In continuing his remarks upon the case of the soldier the last speaker said:

“Now, if this subject is so important to us as this, it becomes necessary for us to know whether it is safe to use vaccine matter one or two removes from the cow or not, AND HE WHO KNOWS YOUNG MEN AS WE KNOW THEM TO-DAY, knows that we cannot take the second remove and vaccinate our children with it with safety. ‘If I vaccinate my child,’ said the doctor, ‘it must be with the bovine virus, and not with any remove, first, second, third, fourth, or any other, from the cow.’”

In answer to which I would say, we do not propose to draw our stock of humanized virus from “our young men, as we know them to-day;” and if you persist that the best we can do is to get it from their children, I hold that if their children have no obvious marks of disease, and vaccination runs its course healthfully and produces a typical vesicle, we have

the absolute proof of millions of cases that with such care as should always be brought to this subject, such virus may be used with perfect safety and success. Of course, in a matter so important, we can never be too careful, and no prudent man would think of using virus from a child whose parents he did not believe to be perfectly free from any constitutional disease. Yet, as I have just intimated, if the child has inherited nothing which interferes with the formation of a typical vesicle, and seems, in every respect, perfectly healthy, having no eruption upon its skin, you need fear no evil, whatever the history of the parents. The transparent lymph from such a source will transmit vaccinia, and that only, in every instance.

At the close of the discussion

“Dr. Jewett suggested the question whether there is any evidence to show that syphilis is inoculable in the tertiary stage by means of vaccination? He did not press a reply, but thought the question was a good one for discussion at some future time.”

I have neither the time nor the knowledge necessary for the full discussion of this question. I have practically taken the negative. The later pathologists take the ground that syphilis, as such, is not hereditary; that the child of syphilitic parents is simply born with a bad constitution—a defective cell-growth—and his pathological developments are no more communicable to others than are those of the scrofulous subject. I believe this is true, but I leave it to others to lay the facts more fully before the Society.

That syphilis may be propagated *with* vaccinia, there can be no doubt. The most numerous examples of this have, as you know, been in the army.

It cannot be very surprising to those who know how wide-spread in every community this terrible disease has become, that the *primary* poison should find its way now and then to a wound like that made for vaccination. This may be by handling, by friction of soiled clothing, or by transmission from one to another by the lancet.

One of the worst chancres I ever treated was on the lower lip of a lady—betrayed with a kiss. The propagation of syphilis, in conjunction with vaccination, is therefore a possible danger at any time, and this fact should make us ever vigilant against every source of contamination. My own view of the matter is that, when syphilis is communicated in this way, it is the primary poison which enters the wound. The cases occurring in civil practice have been so few as to force us to the opinion that they were the result of this accidental infection, or else of gross carelessness. When we consider that millions of cases of vaccination have been performed with humanized virus, with no obvious evil result, and also remember that the operation is not always performed by the skillful and

vigilant practitioner, and further, when we take into consideration the fact that many thousands in this line of succession must have been victims of syphilis, the conclusion is irresistible that syphilis, in its tertiary stage, is not inoculable by means of vaccination. The argument is negative in its character, I know, but the evidence is so cumulative as to leave no room for doubt.

I do not believe it possible to convey to a healthy child any disease but vaccinia, when the virus is absolutely transparent and is taken from a typical circular vesicle on the seventh or eighth day; when there is no surrounding inflammation; when the vesicle has not been previously broken; and when the lymph is taken with such care that you know there is no admixture of blood.

It may be said, here are a great many provisos. That is true; but we are dealing with a subject of immense importance, and ought to be precise, not only in talking and writing about it, but also in its practical details when engaged with our patients, who generally trust us implicitly.

As an under-graduate, I was familiar with the practice of my grandfather and uncle, and humanized virus was their dependence for seventy years. They used it with absolute confidence, often having cases of confluent small-pox under their professional care, and as often escaping personal harm. Part of this experience was in the days when the advent of a case of variola in a small town meant the exodus of all the inhabitants who could get away. I had not been vaccinated since I was six years old, when, at twenty-one, I found myself in a house in Boston, with six cases of varioloid. With child-like faith in my security against the enemy, I remained in the house, which was never properly fumigated or disinfected. I did take the precaution to ask Dr. Bigelow to vaccinate me, but, as it did not take, my immunity, if not accidental, was due to the vaccinia of sixteen years before.

On board ship, bound for California, in 1849, we contracted small-pox at St. Catharine, and, out of one hundred passengers, six had the disease. The ninety-four all suffered a special exposure, and for some reason were not susceptible. This non-susceptibility may have been personal and peculiar in a few; but the many, I believe, owed their escape to vaccination performed from five to forty years before.

During the first and second decades of my professional life, I used humanized virus only, always taken with all the precautions against contamination which I have already mentioned. During this long period I generally had an all-sufficient series of cases to keep up the proper succession, and many of the members of this Society will remember that, from 1860 to 1870, I supplied large numbers of my friends with virus, so that several thousands of cases, for good or ill, are traceable to me, and I am

largely responsible for the good or bad blood that runs in their veins. Of all these cases I have never heard of one that gave rise to the slightest anxiety, except that of a little girl whom I vaccinated in Atlantic Street. In that case there followed immediately a general eruption of pustules, some of which were very slow to heal. Fortunately for my conclusions, I had vaccinated several children on the same day, with lymph from the same source, and all of these cases were typical of perfect vaccine in their entire course. So much exercised was I about the exceptional case, that I invited several physicians to see it. Dr. A. N. Bell examined it with me, and gave it as his deliberate judgment that the virus had nothing to do with the eruption, except as the exciting cause. The child was markedly of scrofulous diathesis. She recovered perfectly at the end of four months.

During the experience of twenty years which I have just detailed, the result of vaccination was uniformly a typical vesicle, very little disturbance of health, and great regularity in the local process. Since 1870 other classes of practice have taken the place of much of my obstetric work and lessened the cases for vaccination. This has driven me to the so-called bovine supply. My experience with this has been that I have vastly more trouble to make it take, and when it does take, so rarely do I get an "old style" typical vaccine vesicle, that I am seldom tempted to take lymph from one of these cases.

Indeed, the principal object I have in addressing the Society to-night is, to raise the most serious question whether we have not been, by our own methods, vitiating and corrupting this most precious boon.

The first rule laid down by your Committee on Hygiene is, "vaccinate with only pure virus."

Now, what is pure vaccine virus? We are all, of course, familiar with the fact that the original source was a vesicle upon the udder of a cow, which had a disease taken in the *natural* way. In the early days of my practice, when I heard occasionally of a resort to the bovine virus, I was simple enough to suppose that somebody hunted around among the fat and lean kine until he found one with the characteristic eruption of vaccinia, and that the market was supplied from this stock. This, however, was not the case; but *calves* were inoculated with what? Sometimes with virus from other calves, and sometimes with the very virus which some suspected had lost its virtue, and the ridiculous assumption reached, that the resulting virus would not only be better than the seed, but as good as the original stock. This, however, does not represent the case in all its absurdity. We know from analogy that the perfection of vaccinia must depend upon its being taken the natural way.

The first method of lessening the virulence of small-pox was by in-

sufflation. How long this practice was in use in India, before the method of inoculation with variolous matter came into vogue, we do not know, but this second method was equally successful, and both methods vitiated the disease, as they were intended to do. Now if we institute an interminable series of inoculations among the calves, how long will it be before the vaccine disease is so modified, or mollified, that we cannot expect it to hold its place as the greatest of all prophylactics?

When we talk of bovine virus and of getting virus from its original source, we deceive ourselves and the truth is not in us, if by these terms we refer to lymph introduced by any artificial method—the original virus being, as you all know, from a *natural* and not an *induced* affection of the cow. Perhaps you will say when introduced into the human system it becomes an induced disease. That is true; but it is so in man from the first, and this may be the reason why it does not deteriorate. We could not know, *à priori*, that it would not lose its virtue, and we might have been tied up to the necessity of using lymph directly from a cow in each individual case. Not from an inoculated cow, but from one taking the disease the natural way. It was only by the experience of forty years that we learned the happy fact, that the lymph produced its kind, absolutely without change, from generation to generation. Again the objector asks: Why should it not produce its kind unchanged when re-introduced to the system of the cow? It would be a sufficient answer to this, if we could give no better, to say that we do not know, but that an abundant experience tells us that it comes back to us so altered that every one notices the difference, both in its physical qualities and in its effects. Fortunately, insufflation and inoculation for the artificial production of variola give us an analogy, a hint, at least, of the difference between an induced and a natural disease. To repeat somewhat for the sake of perspicuity, the lymph from the disease known as vaccinia (whatever the origin of the disease may be) when taken from kine and put into the living man, procures for him absolute immunity from small-pox for an indefinite period, and this lymph is reproduced in the human vesicle unchanged, and capable of indefinite propagation from child to child; but if the cow be subjected to any manner of inoculation, a disease is induced which, *à priori*, we might expect to be as different from vaccinia as in the human system the induced small-pox is different from that taken in the natural way. It may be claimed that this is a difference of *degree* and not of *kind*, but such a claim would be an absolute assumption. We cannot know, if there is a change in the virulence of such an affection, that there is no change in its essential elements. Suppose, however, that it is only a change in degree. Was the original virus too good? Did it produce an immunity too absolute? Was the affection, which for forty

years and more, our fathers trusted in for their safety, too violent? Could we afford to take the risk of vitiating our whole stock by this artificial process, which has gone on and on in commercial circles till doctors and people are alike confounded, and contradict each other on the simplest questions involved in this great subject, which, nevertheless, is so hackneyed that I must needs apologize for taking ten minutes of your time in its discussion.

What virus have we now in use? Probably six or seven kinds.

1. We have, I hope, though I don't know where to obtain it, virus direct from the natural vaccinia.

2. We have humanized virus originally from the natural bovine vesicle.

3. A virus resulting from humanized virus introduced again to the system of the cow, and brought back to man.

4. Probably a virus resulting from inoculation of the cow with small-pox matter.

5. It is not impossible that we have a virus from other vesicular diseases in the cow, resembling vaccinia.

6. We have the calf to calf virus, originally from the true natural kine-pox, and now descended through a long line of induced cases of vaccinia.

In a large number of calves vaccination fails, and this has been, to some extent, attributed to the possible fact that they had already suffered from the kine-pox, and were therefore no longer susceptible. Now suppose in one of these cases the animal's susceptibility was just sufficiently restored to have the virus produce a local excitement, such as it often does in cases of re-vaccination in the human subject, even when the subject is protected from small-pox by his previous experience. Might not this produce a seventh variety—a vaccinoid virus? I certainly should be as suspicious of such lymph as I should of that from a re-vaccination in the human subject.

Dr. Ephraim Cutter quotes, without rebuke, from a letter addressed to him, the following paragraph (see *Trans. Med. Assoc.*, 1872, p. 235): "I got a crust from you several years ago, which was the best matter I ever saw. Out of twenty-four persons vaccinated, twenty took—some with excellent marks of previous vaccination, and one of them, a lady thirty years of age, who had resisted so many efforts at inoculation before, said that she had grown to think that she was not susceptible to the disease. She was a lady in fine health, and the scab from her took well." Now this gentleman, if he is still in successful practice, is perpetuating a virus which I should have no confidence in.

A healthy calf is inoculated and from this artificially induced, and

therefore modified, case of vaccinia, a scab is taken, and a lady who has been previously vaccinated is inoculated therewith—a lady who is so thoroughly protected by her previous vaccination that it is difficult to make any impression upon her; yet, when at last this bad seed germinates in this bad soil, the doctor takes the resulting scab from her arm for general use among the innocents.

In placing my views on record regarding this subject, I ought, perhaps, to say that they are the result of reflection and observation in the ordinary experience of a thirty years' practice, and not the conclusions of an expert, for I have never engaged in any experiments in this line of investigation, and have only the sources of information which are open to all of you. Indeed, I should never have thought of bringing the subject before you again if expressions which I believed to be dangerously false had not gone out to the world from this Society, and been thus far unchallenged. Please recollect that I have had no thought of presenting a general treatise on vaccination; and now allow me to epitomize, in order that no one may mistake the purport of these remarks:

1. I have the utmost confidence in vaccination.
2. I believe humanized virus retains its virtue from generation to generation.
3. I have no faith in any artificial process for obtaining virus.
4. The natural cow-pox is the only source to which we should resort when for any reason we need a new supply.

Dr. Ephraim Cutter, who is excellent authority, says: "The natural cow-pox is enzootic in this country, and only needs looking up to be discovered."—*Trans. Am. Med. Assoc.*, 1872, p. 235.

5. If lymph *only* be used, it can never convey any disease but vaccinia. This, however, does not obviate the necessity for eternal vigilance against accidental admixture, by which primary syphilis, diphtheria, erysipelas, small-pox, etc., etc., may be communicated.

6. Do not understand me as saying that all the cases that have been reported as syphilitic, after vaccination, have been cases of primary infection.

It is a well-settled fact that a *pure* vaccination is often the exciting cause of great disturbance, and of serious manifestations of disease, in those of syphilitic and scrofulous diathesis.

7. Let nothing that I have said be so construed as to convey the idea that there is no protecting power against small-pox in any of the varieties of vaccine virus now in use. So far am I from entertaining this view, that until I can get lymph directly from the natural kine-pox, or in a direct line of human succession from it, I shall use the calf to calf virus, as the next best thing.

8. I honor the gentlemen who, at great pains and expense, have endeavored to supply the profession with virus which they believed was pure and efficient; and though I question its efficiency, and counsel a return to the old and sure method, I shall be glad to see the evidence that calf to calf virus is as good as that from original vaccinia. The burden of proof lies with the advocates of the new product, and I for one shall look for it with deep interest. If they make out a good case, I shall be both delighted and surprised.
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MEDICAL SOCIETY OF THE COUNTY OF KINGS.

The Medical Society of the County of Kings held a regular stated meeting on the evening of Tuesday, Nov. 17th, 1880, at Everett Hall, No. 398 Fulton Street, at eight o'clock, the President, Dr. Jewett, in the chair, Dr. Wyckoff, Secretary.

There were present Drs. Chas. Jewett, J. J. Lamadrid, G. W. Wells, H. G. Small, J. R. Vanderveer, P. H. Kretzschmar, Walter B. Chase, L. Barston Irish, W. W. Reese, J. M. Buell, Benj. Edson, J. H. Hobart Burge, Benjamin Ayers, Benj. F. Westbrook, Alfred S. Houghton, Geo. P. Evans, Henry N. Read, Wm. Gilfillan, J. S. Wight, Wm. H. Thayer, E. R. Squibb, V. Morse, Wm. C. Otterson, G. W. Baker, Jerome Walker, J. D. Sullivan, John Ball, J. A. McCorkle, G. McNaughton, F. H. Stuart J. Healey, A. J. Dower, Wm. Wallace, D. G. Bodkin, A. L. Lowell, A. R. Matheson, P. E. West, C. H. Delavergne, J. S. Young, S. Sherwell, B. N. Briggs, B. S. Vanzile, J. H. Sterling, Geo. Drury, J. Wilson, A. Mathewson, G. K. Smith, F. H. Colton, A. R. Paine, J. A. White, and others.

The minutes of the meetings of the Society for the months of September and October were then read by the Secretary.

DR. WESTBROOK said that in reading the minutes as printed, he did not notice that the names of either Dr. Skene or Dr. Shaw were mentioned as having taken part in the debate on the paper of Dr. Segur, entitled, "The Prophylaxis of Puerperal Convulsions," although their addresses appear in their proper places.

THE SECRETARY said that Dr. Skene's name appears in the printed record in its proper connection.

THE PRESIDENT also stated that the communication of Dr. Shaw was handed in after the meeting and incorporated in the proceedings.

The minutes were then adopted without further objections.

The following proposition for new membership was read by the Secretary :

Dr. J. C. Schapps, No. 153 Ross Street, E. D., Bellevue Hospital Medical College, 1880, by the President.

The Council reported the acceptance to membership of the following duly nominated candidates:

Francis H. Miller, M.D., Univ. of the City of N. Y., 1879; Chas. L. Hogeboom, M.D., Col. Phys. and Surg., N. Y., 1851; Harriet Pillsbury, M.D., Woman's Med. Col., N. Y. Inf., 1880; Anna M. Brown, M.D., Woman's Med. Col., N. Y. Inf., 1880; Edgar Eltinge, M.D., Col. Phys. and Surg., N. Y., 1880:

The first paper of the evening, entitled, "Diseases of the Glands of the Female Urethra," was read by Dr. A. J. C. Skene.

DR. WESTBROOK said he would like to ask the author of the paper one question in regard to treatment. The doctor says that when the probe-pointed scissors are introduced into the duct or glandule, the duct is slit open on the posterior side. He would like the doctor to state more particularly what he meant by the posterior side.

DR. SKENE, in explanation, said: From the duct toward the vagina he laid the wall of the duct open from within to the wall of the vagina. That is the posterior wall when the patient is lying on the left side. The incision is made from the urethra towards the vagina.

The second paper, entitled, "A case of Chronic Empyema with Extensive Amyloid Degeneration, with Remarks on the Subject of Amyloid Degeneration," by Drs. P. H. Kretzschmar and B. F. Westbrook, was read by Dr. Westbrook.

DR. EVANS, at the conclusion of the paper, said: Dr. Westbrook stated in the paper that Rindfleisch is of the opinion that amyloid degeneration commences primarily in the hepatic cells. He, Dr. Evans, was inclined to believe that unless Rindfleisch had changed his views since 1875, Dr. Westbrook had made a mis-quotation. In Dr. Evans' edition the author quoted states that the degeneration began in the arterioles primarily, and secondarily in the hepatic cells.

The third paper, entitled, "Brief Review of the Society's late Discussion on Vaccination, with Remarks on Humanized Virus, and on Non-inoculability of Tertiary Syphilis," was read by Dr. J. H. Hobart Burge.

DR. BURGE, before entering upon the reading of the paper, called attention to the fact, by way of explanation, that the title of his paper did not agree with that noted upon his card. The announcement should have been, "A Brief Review of the Society's late Discussion on Vaccination: with Remarks upon the Virus now in Use." The question of non-inoculability of tertiary syphilis not forming a prominent feature of his paper, he preferred should not be put in the title; but by some means or other it got into the title, as announced; he preferred to keep it back.

The paper was then read.

DR. KRETZSCHMAR said that as he probably was the first "impersonality" pointed out by Dr. Burge in his paper, he would, with the permission of the Chair, take the liberty of stating his side of the question. Dr. Burge mentions the unfavorable experience which the speaker had had, as per the debate referred to, with humanized virus; and that he (Dr. B.) expected Dr. K. to join the Anti-Vaccination Society in New York if he should have similar experience with bovine virus!

He was not very certain whether Dr. Burge was present at the time the debate on vaccination was held or not, or whether he read the remarks made on that occasion carefully; but if he had not, he would repeat what he said then. He was in need of vaccine, and, meeting a prominent member of this Society, in whom he had all confidence, he obtained from him three sticks of humanized vaccine; and, after he had used two of these sticks, with a supposed clean instrument, he found that both of the cases developed erysipelas. One of them recovered, the other died. He had since vaccinated perhaps one hundred children with bovine virus, and he had seen nothing like erysip-

las in any of them ; and, under the circumstances, he felt that it was fair and proper to mention the case. He desired to say that it was not an accident which occurred because of want of cleanliness of the instrument ; it was simply his experience with a number of cases occurring in his own practice.

He would mention, at the same time, that he had used the same knife every time since, having never used any other knife for the purpose.

DR. BURGE replied that he was not present on that occasion, and merely took the record from the PROCEEDINGS.

DR. OTTERSON said there was a reference in that debate to some cases which occurred under his observation while in the army. The results of these cases are due to imperfect or bad teaching in regard to humanized virus. It is very well known, of course, that a great many gentlemen who were connected with the army professionally at that time would have done better service as officers or as bearers of muskets. Very little care was taken or judgment exercised as regards vaccine ; and anything which was supposed to be, or what purported to be, vaccine, was eagerly seized upon, in order to protect those who were supposed not to have been protected. In the case in question vaccine was taken from the arm of a young man, a soldier, and a private, who had been vaccinated before, so that this was the second vaccination. During the process of development of the vaccination, he contracted syphilis, and presented himself in the army with the scab. He told the surgeon that while in St. Louis he had been vaccinated with matter from what had been deemed by the regimental surgeon to be a good source ; and it was deemed best, without consultation and without sufficient information, to secure this scab, which was used quite freely throughout the regiment.

According to the best of his recollection, there were in the regiment some six hundred men, one hundred of whom were entirely disqualified for duty and were discharged on proper certificate.

It is too long a story to detain the Society with, in the recital of the symptoms which presented themselves.

The point simply is that the teaching was—for there was then no bovine virus in this country—that humanized virus was good ; that was the theory.

At that time it was his good fortune to be associated with Dr. Martin, who then used freely of humanized virus, and depended upon it entirely. Recently, he had noticed, the National Board of Health preferred the humanized virus, and it is used extensively throughout the British Kingdom.

The Society then passed to executive business.

DR. OTTERSON arose to a question of privilege, and called attention to certain papers which had been distributed in the seats of members, calling attention to an opportunity to secure instruction in the German language. He extolled the abilities of the teacher, Dr. Deutsch, and expatiated on the benefits to be derived from an intimate knowledge of a language in which so many valuable medical books are written.

THE PRESIDENT called for the report of the Committee in the matter of the late J. Sullivan Thorne, deceased, but as there was no response, the matter was again laid over for one month.

In connection with a second call for applications for membership, the President called attention to a blank which had been prepared by the Secretary, for use by members in proposing candidates for membership. He requested its use hereafter. Cards may be had of the Secretary, or they will be found at his table on the evening of the meeting of the Society. The blank is as follows :

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

I desire to nominate [and recommend*] for Membership in this Society

DR.....

RESIDENCE, No.....

Place and Year }
of Graduation, }
if known. }

(Signed)..... M.D.

Member of the Medical Society.

*Erase these two words when a recommendation is not intended.

THE CHAIR announced the death of Dr. William Ross, a member of the Society.

DR. WYCKOFF moved, and it was carried, that a committee of three be appointed to take proper action in regard to the death of Dr. Ross.

THE CHAIR requested Dr. Wyckoff to nominate the committee.

DR. WYCKOFF thought that matter could very well be left to the Chair, and requested him to appoint the committee in the usual way.

THE PRESIDENT thereupon appointed Drs. D. G. Bodkin, J. Griffin and J. S. Young, as such committee.

Under the call for new or unfinished business,

DR. WALKER asked the Chair whether the committee known as the "Medico-Legal Committee" had done anything in relation to the matters entrusted to their care?

THE PRESIDENT, in response, called upon the Chairman of that Committee, Dr. Sandford, for information. There being no response, he asked any member of the Committee if the Committee were ready to report?

DR. J. S. WIGHT, a member of the Committee, replied, that there was no report at present, either of progress or anything else, so far as he knew. The Chairman may have something, but he knew nothing of it.

A motion to adjourn was then made; but before putting it

THE CHAIR asked whether the committee appointed at the last meeting of the Society to procure statistics in relation to puerperal convulsions were ready to report concerning their labors?

There was no response.

The Society then, at 10.30, on motion, adjourned.

Ἀσκληπιὸς



ὁ Σωτήρ

Χάρμα μέγ' ἀνθρωποῖσι, κακῶν θελκτῆρ' οἰσυνάων.

Hymns of Homer, No. XVI.

PROLIFERATIONS.

—THE TRANSACTIONS OF THE MEDICAL SOCIETY OF THE STATE OF NEW YORK FOR 1880 are in the hands of the TREASURER, of whom copies can be obtained.

—THYMOL is said to have the property of immediately removing the odor of tobacco.

—FOR THE BENEFIT of our more conservative brethren we arrange in alphabetical order the names of some of the newest of the great remedies presented to the profession: areca, ava, bael, berberis, boldo, cercis, coto, chaulmangra, goa, gurjun, hoang nung, penthorum, querbracho, sumbul, sundew and tonga (from the Fijians). There is no truth in the report that these names are copyrighted by an inmate of a Western lunatic asylum.

—ESMARCH'S POWDER.—The so-called painless caustic powder of Esmarch is prepared as follows: arsenious acid 1 part, morphia sulphate 1 part, calomel 8 parts, pulv. gum arabic 48 parts; mix.

—THE GREAT FAMINES, pestilences and general tribulations that have been predicted as due in the current year are now reported as postponed, on account of the weather, until the year 1881. In point of fact, the meteorological conditions have been decidedly unfavorable to the wholesale misery and suffering that was set down for the present year.

—GLYCERINE IN GASTRIC FLATULENCE, ACIDITY AND PYROSIS.—Glycerine does not prevent the digestive action of pepsin and hydrochloric acid; hence, whilst it prevents the formation of wind and acidity, probably by checking fermentation, it in no way hinders digestion. One or two drachms may be taken either before, with, or immediately after food; in water, coffee, tea or lemon and soda water. In tea and coffee, it may replace sugar, a substance which greatly favors flatulence, as, indeed, does tea in many cases. In some instances a cure does not occur till the lapse of ten days or a fortnight.—DRS. SYDNEY RINGER and MORRELL.

—BENZOATE OF SODA IN DIPHTHERIA and scarlet fever has been tried by Demme, at the Children's Hospital at Berne. He has used it both internally and externally. He also uses brandy in septic cases. Of twenty-seven cases, he lost six—a favorable result, he considers, in view of the severity of the attacks. . . . The benzoate has also been used with successful results in whooping-cough.—*London Med. Record*, Aug. 5.

—DIPHTHERIA IN RUSSIA.—The measure that has been found most efficacious in arresting the spread of diphtheria in Poltava, has been the fumigation of infected premises with burning sulphur. The peasants, supported by the priests, object to this process. They regard it as impious and contrary to the rites of religion. Moreover, they hold it to be foolish. The treatment of the sick person they can understand, but not that of the house. "Cure the sick, if you like," they say, "but what have our houses to do with sickness?" They resist the disinfectant in some places, and in one village they were only prevented by the police from burning alive a sister of charity, a fire having been kindled for that purpose.

—TREATMENT OF BURNS.—At St. Francis' Hospital, New York, Dr. G. F. Shrady is in the habit of using the following dressing for burns and scalds: gum acacia 3 oz., gum tragacanth 1 oz., carbolized water [1-60] 1 pint, and molasses 2 oz. Apply with a broad, flat camel's-hair pencil. This plan of treatment is essentially that of the late Dr. Gurdon Buck, and really has very many advantages.

—JEFFERSON AS A VACCINATOR.—The January number of the *North Carolina Medical Journal* will contain the Story of the Introduction of Vaccination in "The South;" consisting of a series of letters written in the year 1801, by Thomas Jefferson, President of the United States, to Benjamin Waterhouse, the Jenner of America; and some slight comments thereon by Henry A. Martin; illustrated by exact *fac simile* copies of all the letters.

—VACCINE VS. ECZEMA.—It is the opinion of many physicians that eczematous children should not be vaccinated. The *British Med. Jour.* has had several communications claiming that this is a mistaken opinion, and giving reports of cases showing the curative influence of vaccination upon infantile eczema. In the issue of that journal for Sept. 18, Dr. J. C. Murray narrates that in a severe case of eczema capitis, that had defied ordinary treatment, he practiced vaccination with the result of obtaining a complete cure.

——“DRS. READ, HUNT AND FRENCH, with the Librarian *ex-officio*,” should have been the reading of the Report of the Library Committee, on page 324 of the November PROCEEDINGS.

——COBBOLD ON ENTOZOA (an *edition de luxe*) is the latest addition to the Library.

——APHORISMS FROM THE FRENCH. (*L'Union Méd.*)—When one considers the credulity of the public in relation to medicine, the wonder is, not so much that there are so many quacks, as that there are still so many honest persons among the doctors.

——A WELL-KNOWN lady of fashion asked her physician how many doctors it would take to make a scholar? He replied, just as many as it would take of lovers to satisfy a coquette.

——CAPTAIN EADS' SHIP RAILWAY.—The *Scientific American* of Nov. 3 contains two full page illustrations of Captain Eads' proposed railway for transporting ships with their cargo across continents. Captain Eads claims by his plan to be able to take loaded ships of the largest tonnage from one ocean to the other across the Isthmus of Panama, as readily as can be done by a canal after the Lesseps plan, and at a much less cost for engineering construction. The project is certainly bold and ingenious, and the projector anticipates no serious difficulties in carrying forward his enterprise. The engravings referred to in the *Scientific American* show the proposed construction of not only the railroad, but the appliances for transferring the ships from the water to the rail.

——AFTER THE LAW now in force requiring the registration of medical practitioners has had a fair trial, and thus been allowed to show, as it most probably will, that it is of no restraining value whatever, a *real* step in advance should be made by the enactment of a law to the effect that no one thereafter shall practice medicine in any of its branches (existing rights being, of course, respected), who shall not have passed a satisfactory examination, and been licensed by a State Medical Examining Board. Medical degrees granted after the introduction of this Reform would be merely ornamental titles, so far as the right to practice in this State, by virtue of them, is concerned, and might then be freely and even lavishly conferred by each school at its own sweet will, with no one to molest or make it afraid. This liberty would not be unduly taken advantage of, for any school whose graduates always passed the State Board would, *ipso facto*, draw students to itself by the assurance so given that the teaching was good. In Germany no one can legally practice medicine, or call himself physician (*arzt*), unless he has passed the “Staats-Examen.”—J. S. P.

SEE PAGES 377 and 380.

—THE REGULAR MONTHLY MEETINGS of the Medical Society of the County of Kings are held at 8 P.M. on the third Tuesday of each month, at Everett Hall, 398 Fulton Street.

The December meeting will be held on the 21st, at which the following papers will be presented:

The Surgical Uses of Wire Cloth, with Practical Demonstrations of the Method of Making and Applying Surgical Dressings of Wire Cloth, by Dr. J. S. Wight.

Treatment of Aneurism with Potassium Iodide, with presentation of patient, by Dr. B. F. Westbrook.

Diseases of Shop Girls, (notes from private practice), by Dr. D. E. Chace.

At this meeting Nominations will be made for Officers of the Society for 1881.

MEDICAL SOCIETY OF THE COUNTY OF KINGS.

OFFICERS AND COMMITTEES FOR 1880.

<i>President.</i>	C. JEWETT, M.D., 307 Gates Ave.
<i>Vice-President</i>	G. W. BAKER, M.D., 48 Bedford Ave.
<i>Secretary</i>	R. M. WYCKOFF, M.D., 532 Clinton Ave.
<i>Assistant-Secretary</i>	J. H. HUNT, M.D., 419 Hart St.
<i>Treasurer</i>	J. R. VANDERVEER, M.D., 301 Carlton Ave.
<i>Librarian</i>	A. HUTCHINS, M.D., 796 De Kalb Ave.

CENSORS.

B. A. Segur, M.D., 281 Henry St.	F. W. Rockwell, M.D., 6 Lafayette Ave.
E. R. Squibb, M.D., 36 Doughty St.	L. S. Pilcher, M.D., 4 Monroe St.
J. D. Rushmore, M.D., 129 Montague St.	

DELEGATES TO THE MEDICAL SOCIETY OF THE STATE OF NEW YORK.

(1878 to 1882.)

Drs. J. C. Shaw,	Drs. G. G. Hopkins,	Drs. J. Byrne,
J. D. Rushmore,	J. S. Wight,	B. F. Westbrook,
R. M. Wyckoff,	A. Sherwell,	E. N. Chapman,
A. Otterson,	W. Wallace,	F. W. Rockwell.

Chap. XI., Art. 2, of By-laws: "Any Member elected as Delegate to the Medical Society of the State of New York, who shall be unable to act as Delegate during two successive years, shall be considered to have vacated his position as Delegate."

COMMITTEES OF THE SOCIETY.

HYGIENE.

Drs. J. Walker,	B. Edson,	N. B. Sizer.
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REGISTRATION.

Drs. W. E. Griffiths,	Drs. W. G. Russell,	Dr. R. M. Buell
A. S. Clarke,	N. Matson,	
J. A. Jenkins,	F. W. Rockwell.	

PHYSICIANS' MUTUAL AID ASSOCIATION.

Drs. B. A. Segur,	W. W. Reese,	J. H. H. Burge,	A. Hutchins,	W. G. Russell.
I. H. Barber,		A. W. Catlin,		

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